THE AMERICAN NATURALIST

EDITED BY JAQUES CATTELL

A SEMI-MONTHLY JOURNAL
DEVOTED TO THE ADVANCEMENT OF THE BIOLOGICAL SCIENCES

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EDITORIAL

THE AMERICAN NATURALIST with 1946 begins a new editorial policy.

Originally the new plan called for a large monthly issue followed by weekly, eight-page, news supplements. For the present we have decided to publish only twice a month—a large issue on the first of the month, to be followed on the fifteenth by a 16-page, news issue. The projected change is being made because of the widely expressed need for prompt publication of biological news. News items of interest to biologists will be cordially welcomed, also articles of a less technical nature not exceeding 2,000 words in length. Included will be a section for Letters to the Editor, where current events can be freely discussed.

The general complexion of the longer research contributions will, for the present, be devoted, as in the past, to Genetics and Evolution. These articles will appear monthly and will have as many words per year as before.

An editorial committee will be appointed at the coming March meetings.

Concerning the changes the editor has obtained a cross section of opinion through the letters received. They are so much in favor of the new plans that we are proceeding with great confidence. The following pages include all letters received to date.

The form of this journal should appeal to individual subscribers and for the success of the journal we shall need support through a large number of subscriptions. The old AMERICAN NATURALIST had a subscription list of slightly under a thousand. The semi-monthly issue should have a subscription list of at least five thousand.

There is great need of integration in biological fields. This will be aided by increasing mutual understanding through the presentation of the progress of new developments, including those of special interest to the biologist in political and international matters.

THE AMERICAN NATURALIST will have served its purpose well by rallying the forces representing the biologists of the country and by serving as a mouthpiece that can be quoted by newspapers at large.

The neglect of biology as a basic science has been emphasized by the fact that the original science bills being prepared for Congress, also the Bush Report ignored biologists as a group. Fortunately, a good deal of this is being corrected as exemplified by the article on page 17 by Dr. Robert Chambers on Recent Developments in Science Legislation. The Editor urges every one who reads this editorial to contribute further discussions, so that the subject may be thoroughly aired, making it possible for biologists to become influential in matters coming up for consideration by the Federal Government.

The Editor welcomes suggestions and criticisms, so that a sound editorial policy may be evolved having the full support of biologists.

May we have your full support?

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LETTERS TO THE EDITOR ON THE CHANGE IN POLICY

THE AMERICAN NATURALIST has gained a high position among biological journals during its long and honorable history. Although its editorial orientation has been changed from time to time, this flexibility gives only a partial background for your projected hazard of new fortunes. It will be interesting to find whether the increased attention being given to science and scientists and the growing importance of scientific work for the general public will justify your enterprise. More precisely put, the problem centers about the need for a privately owned and edited journal operating near a portion of the field occupied by *Science* in its capacity as one of the official organs of the American Association for the Advancement of Science. Aggressive liberalism—such as was frequently shown by your distinguished father, the late J. McKeen Cattell, in his editorial policy for *Science*—is needed now in editorial circles even more than in the recent past. It is my belief that you will continue in this tradition of liberalism. In so doing you may expand the usefulness of THE AMERICAN NATURALIST and indirectly exert an influence on the reporting of news about science and scientists in other journals and in the general press. I hope your new venture will succeed.

W. C. ALLEE

THE UNIVERSITY OF CHICAGO

THE AMERICAN NATURALIST has had a long distinguished career, and its various changes have mirrored changes in biological thought. Your new proposals should make it an even more accurate reflection of current biology. I have every confidence that the new mutation will be a progressive one, rather than sub-lethal.

EDGAR ANDERSON

MISSOURI BOTANICAL GARDEN,
ST. LOUIS

Your plan to publish intermediate weekly issues of THE AMERICAN NATURALIST strikes me as being an interesting and useful departure. I trust the effort will succeed. I shall wish to be one of the subscribers.

L. H. BAILEY

CORNELL UNIVERSITY

I have your letter of November 8th, and I am greatly interested in the new plans for THE AMERICAN NATURALIST. I hope everything works out for the mutual interests of the biologists and for you as publisher. With cordial personal greetings,

GEORGE A. BAITSELL

YALE UNIVERSITY

I am certainly interested to hear that THE AMERICAN NATURALIST is to appear in a new format and to become a weekly instead of a monthly as it has been so long. Needless to say, it has my best and warmest good wishes for a bright and brilliant future. With kindest regards to you,

T. BARBOUR

HARVARD UNIVERSITY

I have read with interest your announcement that THE AMERICAN NATURALIST is going to have weekly issues giving news of biological interest. It seems to me that there is a place for such a weekly publication. I find that many people are interested in the brief notes which appear in *Science Newsletter*, but they often express regret that several pages are not given to a non-technical review. In short *Science Newsletter* is too brief for many readers who have a general interest in biology. It is my opinion that even many professional biologists will welcome articles which are of a non-technical nature and which give an accurate summary of current biological events. It is absolutely impossible for a specialist to keep track of what is going on in the various subdivisions of biology to-day.

MAURICE A. BIGELOW

COLUMBIA UNIVERSITY

Your communication concerning plans for changing the make-up of THE AMERICAN NATURALIST came to hand many days ago; through carelessness it got mislaid, and thus neglected.

However, I do not feel my estimate (or that of anyone else, for that matter) on a pretty-much unknown quantity or quality can be seriously regarded. I easily remember the time when, years ago, it was decided to appropriate THE NATURALIST for the essentially sole func-

tion of broadcasting results in the field of genetics research; that was, indeed, a fast-growing and leading area of biological interests at the time; and such procedure was probably justified even though such use was certainly not what would naturally be suspected in a medium with such broad titular designation. At present that department of research, while still scientifically important, has passed the zenith of its interest,—at least, as it seems to me; and the journal may well resume more of the nature its name implies. Just what character it may well take depends on how available the lines of information you briefly outline are already sufficiently available in other publications. Both *Science* and its running mate, *School and Society,* give much news of current happenings and personalities. *The Scientific Monthly* covers many topics of what you term "less technical nature." Of course *The Quarterly Review of Biology* (the one originated by Pearl is what I mean) is confined to biological considerations, with special attention to an evaluation of new books. *Physiological Reviews* has its field; and there is *The Journal of Ecology,* as well as others specializing in almost every field, tropical medicine, parasitology, etc. However, science covers an almost infinite number of "facets" and interests; there is an infinite number of folks who like to write and publish. I think your proposed renovation of THE AMERICAN NATURALIST will doubtless command wide circulation on its own merits, although this immediate sentence should not be quoted without mention of the question mark implied in preceding paragraphs. With all good wishes for success,

R. A. BUDINGTON

WINTER PARK, FLA.

Your plan to change the nature and scope of THE AMERICAN NATURALIST is of interest. There seems to be no adequate biological magazine designed for educated general readers as well as for professional biologists. Indeed, the highly worthy achievements in physical sciences during the war years may have served to obscure the tremendous importance of the life sciences. It seems to me to be a safe guess that the real importance of biological sciences will be appreciated from now on by large numbers of the reading public. Life and its processes are of supreme importance. The NATURALIST has done a worthy but restricted job. If you really enter upon your new proposal I wish for it an initial and a rapidly increasing acceptance. You know what the problems are in making such a proposed magazine succeed financially. It can not succeed financially unless it succeeds educationally. Hence your greatest effort must be given toward making a magazine that is outstanding in quality and usability. I wish you success and herewith enter my subscription for two years, for which please send me a bill when and if you reach that point.

OTIS W. CALDWELL

BOYCE THOMPSON INSTITUTE,
YONKERS, N. Y.

I have your letter concerning your ideas about THE AMERICAN NATURALIST. I heartily subscribe to the revamping of the Journal and I am sure it would fill a great need in the biological world. I see no reason why such changes would not meet with enthusiastic reception on the part of the society. I shall be glad to continue serving as a member of the Editorial Committee.

J. H. BODINE

STATE UNIVERSITY OF IOWA

I think that the format of THE AMERICAN NATURALIST fulfils a useful purpose, and I am very glad to contribute, if and when I am able to do this. The only embarrassing point is that "expressing good wishes" seems to mean a new subscription—and while I definitely do wish you good luck, I am not at all sure that I should enter a subscription. However, that is not proposed at present, and I shall leave the subject by expressing good wishes.

S. C. BROOKS

UNIVERSITY OF CALIFORNIA,
BERKELEY

I am greatly interested in your plan for the future of THE AMERICAN NATURALIST. For many decades this journal had occupied a unique position among biological publications as one which every biologist could read from cover to cover with profit and satisfaction. At the present time, when the several biological fields are being narrowed and multiplied, a journal of this kind is particularly valuable. I wish you all success.

CHARLES T. BRUES

HARVARD UNIVERSITY

I am very interested in your plans for a revamping of THE AMERICAN NATURALIST, by

which a large monthly and brief weekly issues will be produced. The weekly issues, concerned with news of biology and biologists, will be something entirely new, and should prove to be of value to all biologists. I am looking forward to this new source of information concerning biologists and their work with great interest, and extend to you my most cordial wishes for success in your venture.

ASA C. CHANDLER
THE RICE INSTITUTE

Thanks for your letter about THE AMERICAN NATURALIST. I think your plan is worthwhile and I will expect that this journal in its new form will fill a needed place in biological literature. I hope it will succeed.

HENRY A. CHRISTIAN
BROOKLINE, MASS.

I am very much interested in your proposed weekly issue of THE AMERICAN NATURALIST, for I believe there is a need of such a journal. At the present time the interest in the physiological and medical aspects of biology overshadows both zoology and botany in their more simple aspects, what may well be called their natural history as well as their systematic side, and I hope that your plan may work out to meet this need. There is real danger that in the ever-widening fields of applied biology we shall overlook or ignore the animals and plants that make up the biological world. A journal such as you propose ought to be a real help in saving us from such a blunder. With the best of wishes for the success of your undertaking.

HUBERT LYMAN CLARK
HARVARD UNIVERSITY

Your letter of November 20, about THE AMERICAN NATURALIST, leaves me somewhat puzzled. I do not, at this time, quite understand the purpose of the weekly issue. Would there be enough news to justify it? And who are the biologists? Judging by the current journals, they are understood to be the laboratory men, and others are little regarded. I have always felt with regard to *Science* that it was a pity that (owing to the limits of space) so many good books failed to be reviewed. I don't know whether the new THE AMERICAN NATURALIST will help in this matter. It may be that you are beginning a service which will be of great value and importance. I hope so. As for myself, I no longer pretend to keep up with biological literature and events, as I used to do.

THEODORE D. A. COCKERELL
UNIVERSITY OF COLORADO

It will be a pleasure to receive a few copies of your new weekly THE AMERICAN NATURALIST. I shall then be able to write you a letter about the new plan. It sounds good.

WILLIAM H. COLE
RUTGERS UNIVERSITY

Of course you have my good wishes for the new THE AMERICAN NATURALIST, although I do not feel that I am closely enough in touch with the history of this journal nor with your plan to be able to write you a letter that could be usefully printed. I regret very much that owing to extreme pressure of work I am having to decline practically all the requests I receive to write or speak for various groups and publications at the present time. With best wishes,

GEORGE W. CORNER, M.D., Director
CARNEGIE INSTITUTION OF WASHINGTON,
BALTIMORE, MD.

Your letter of November 20 in regard to changes in the editorial policy of THE AMERICAN NATURALIST has been received. I was very much interested in your proposed changes in this journal and wish you every success in your new venture.

W. W. CORT
THE JOHNS HOPKINS UNIVERSITY

I am interested to learn that you are planning a change in THE AMERICAN NATURALIST, both in frequency of issue and in contents. The weekly issue containing news of biologists and of biological interest will certainly be helpful. And the larger monthly issue can, under your skilled guidance, play a very important part in bringing home to the more thoughtful people in this country the rôle of the life sciences in the kind of civilization that is upon us. You have great inherited talent in such matters and I predict for the new THE AMERICAN NATURALIST a long life of great usefulness to many.

E. V. COWDRY
WASHINGTON UNIVERSITY

I was much interested in reading of the new plans for THE AMERICAN NATURALIST. They sound very excellent and I hope and trust that

all that you have visualized will come true. Please accept my very best wishes for the future of the new THE AMERICAN NATURALIST and may it continue to be one of the best publications upon the subjects of which it treats. With best wishes to you personally for a Happy Christmas and a Prosperous New Year, I am,

CHAS. F. CRAIG,
Colonel, U. S. Army, Retired,
Editor, The American Journal of Tropical Medicine

Your father used to remark frequently when we were introducing or being introduced at scientific meetings that he and I were "sort of cousins"; the fact of the matter was that he was a second cousin of the second wife of Mrs. Curtis' uncle. I always appreciated his very friendly attitude toward me, which was manifest on many occasions and began when I really became acquainted with your father and mother, the summer you were all at Woods Hole and when Eleth and McKeen took the Invertebrate Course. I have mailed my check in payment of subscription to the enlarged NATURALIST. I want to congratulate you upon the plan. In view of the Cattell genius for making a success of publications, financially as well as editorially, I am sure the new venture will succeed. With personal regards,

W. C. CURTIS

UNIVERSITY OF MISSOURI

This is a bit delayed answer to your letter of November 3d in re THE AMERICAN NATURALIST. I can only wish you success in your new undertaking, or rather in the new modification of the old undertaking. Although your letter is too laconic to give a clear idea of exactly what the new NATURALIST will be like, the general features of the project appeal to me. I shall be glad to be helpful to you as a member of the editorial committee.

TH. DOBZHANSKY

COLUMBIA UNIVERSITY

I was delighted to learn that, starting January 1st, THE AMERICAN NATURALIST will become a weekly journal. I should like to take this opportunity to extend to you my very best wishes for the success of the new THE AMERICAN NATURALIST.

VINCENT DU VIGNEAUD

CORNELL UNIVERSITY MEDICAL COLLEGE

I am distressed at the gross change in policy for THE AMERICAN NATURALIST. I see no reason for overlapping the functions of *Science* in editorial policy. Your form letter strongly suggests an attempt to turn THE AMERICAN NATURALIST into a popular scientific journal with probable loss of standards in the field of evolution. It looks like an attempt to make money without considering the place of the journal in scientific fields. In my opinion, THE AMERICAN NATURALIST has been the leading journal in the field of evolution. No other journal takes its place. Other journals do fill the need for general scientific news and general popular articles. One of my prime interests is the field of evolution. I suspect that I shall not care to subscribe to THE AMERICAN NATURALIST if the policy is changed along the lines you have laid down. I shall make my determination after examining the first issues.

ALFRED E. EMERSON

UNIVERSITY OF CHICAGO

Your plan for a new AMERICAN NATURALIST sounds splendid. A magazine of general information for all biologists should find a very welcome spot. Good wishes to you in your new departure.

THOMAS FRANCIS, JR.

UNIVERSITY OF MICHIGAN

The proposed expansion of THE AMERICAN NATURALIST to include brief articles on topics of general biological interest will presumably infuse new life into our venerable journal which never yet has faltered in attacking problems of growth and evolution. Now, while biology is rapidly expanding its scope, readers of the NATURALIST, in my opinion, will welcome these proposed sidelights, which may help to keep in view the whole evolutionary field.

JOHN H. GEROULD

UNIVERSITY OF NEW HAMPSHIRE

I am interested to learn of your plans to enlarge the scope of THE AMERICAN NATURALIST. In these nuclear days and in days yet to come, the life sciences have an important social role. A journal which takes in the whole realm of biology (not excluding the human infant!) can perform a constructive public service. All good wishes to your undertaking.

ARNOLD GESELL

YALE UNIVERSITY SCHOOL OF MEDICINE

Your plan to make THE AMERICAN NATURALIST representative of all biologists is an excellent idea. General and shorter articles which you are planning to publish in the weekly issues will be tremendously appreciated by those of us who find it difficult to follow the more technical advances in genetics and evolution. The new THE AMERICAN NATURALIST will, no doubt, be welcomed and supported by a great number of biologists. I wish you the very best of success in your venture.

ROBERT GESELL
UNIVERSITY OF MICHIGAN

Thank you for your letter. It is a good thing to try something new and I wish you good success. The old friends and contributors to THE AMERICAN NATURALIST will of course appraise the pudding at the time of eating it. Let us hope that we are in for a fine treat.

R. B. GOLDSCHMIDT
UNIVERSITY OF CALIFORNIA

The natural history of an animal or plant, that is, a study of its life as lived in the wild among its friends and enemies, is fundamental for all other work in botany or zoology. I have long felt that the greatest need for a scientific publication in the United States was for one which would publish high-grade natural history notes and articles. THE AMERICAN NATURALIST in the old days met that need. It then backslid into genetics and heredity. I am glad to see that it is recovering. The natural history section of the new NATURALIST will go far toward meeting the need that has long existed and I wish you the greatest success in this your venture.

E. W. GUDGER, *Librarian,*
Dean Memorial Library
AMERICAN MUSEUM OF NATURAL HISTORY

There is need for such a journal as that which you propose. It should receive whole-hearted support from a large number of biologists. I wish you the success which it deserves.

FRANK A. HARTMAN
THE OHIO STATE UNIVERSITY

I should like to write you a letter expressing good wishes for the new THE AMERICAN NATURALIST. However, I am not sure that I like the idea of a weekly journal containing news of biologists and articles of too popular a nature, especially if it is also to carry technical articles on genetics and evolution. I believe I would prefer to see it continued under its old policy.

E. NEWTON HARVEY
PRINCETON UNIVERSITY

The publication of THE AMERICAN NATURALIST as a weekly journal, containing current information in the biological field in addition to longer articles of the same broad scope as in the past, should make the journal of even greater interest to its previous readers. As a subscriber to the journal for over thirty years I am glad to join with others in best wishes to you and THE AMERICAN NATURALIST.

H. K. HAYES
UNIVERSITY OF MINNESOTA

I wish I knew what to say in answer to your letter about THE AMERICAN NATURALIST. Quite truthfully, I haven't looked inside the covers of the NATURALIST for several years, assuming that it was mostly concerned with genetics and evolution. Are you proposing in the transformed journal to print general articles of a kind that we discussed, and are you planning to use this as competition to *Science?* If you are, my guess is that you will deliberately have to solicit articles from specific individuals on specific topics. In any event, good luck to you on your new venture and may it do something to knit the scattering branches of biology into a more continuous texture.

SELIG HECHT
COLUMBIA UNIVERSITY

I was very much interested in the contents of your letter of November 27th and your revised plans for THE AMERICAN NATURALIST. I see a real possibility in this plan. As the journal becomes more and more cosmopolitan in its acceptances it may serve as a unifying organ for the biologist. I wish you every possible success in this venture and hope that contributors in every sub-field within the biological sciences will take advantage of this opportunity to present general contributions of a less technical nature to their colleagues in other fields.

LEIGH HOADLEY
HARVARD UNIVERSITY

I am pleased to learn of your plan for enlarging the scope of THE AMERICAN NATURALIST.

We need more facilities for the publication of nontechnical articles of general scientific interest. I wish you every success in your new enterprise.

S. J. HOLMES

UNIVERSITY OF CALIFORNIA,
BERKELEY

I have your note of November 27th, and thank you for including me among the men to whom you have written. I think your plans for THE AMERICAN NATURALIST are mighty good and I wish you every success. I am quiet here in Bronxville, at the age of 88, but I expect to go back to Washington in the fall. I will keep you posted as to my address.

L. O. HOWARD

BRONXVILLE, N. Y.

The news regarding the publication policy of THE AMERICAN NATURALIST is interesting and good, and I appreciate your writing on the subject. I think the idea to be an excellent one in principle and in detail. I, of course, will continue my subscription and I think most other present subscribers will do likewise. I shall be happy to continue to serve on the editorial committee and suppose other committee members will do likewise.

CARL L. HUBBS

SCRIPPS INSTITUTION OF OCEANOGRAPHY

Your description of the new plans for THE AMERICAN NATURALIST suggest to me that it will be a journal which will fill a broad and important need. I wish it much success.

TRUMAN L. KELLEY

HARVARD UNIVERSITY

I am indeed pleased to learn that THE AMERICAN NATURALIST is to be increased in both size and frequency of issue. I sincerely hope that you will be able to run a journal of semi-popular nature, making available to the general public the progress in biological science in a fashion which will gain a greater support for this work from the public, as well as keep them informed as to the nature of progress in the field. I extend my best wishes for your new venture.

CARNEY LANDIS

COLUMBIA UNIVERSITY

We are happy to know that THE AMERICAN NATURALIST will be issued weekly beginning in January. You may be proud of this fine journal. Here's hoping that it will continue to your full satisfaction.

CHAUNCEY D. LEAKE

MEDICAL BRANCH AT GALVESTON
OF THE UNIVERSITY OF TEXAS

I am happy to congratulate you on the proposed expansion of THE AMERICAN NATURALIST, the oldest in America, I believe, of our strictly biological journals. The new venture has my best wishes for its success, and I am confident that under the aegis of the Jaques Cattell Press it is assured of high standards and will go on to still greater usefulness.

FRANK R. LILLIE

CHICAGO, ILL.

The new approach for THE AMERICAN NATURALIST holds tremendous possibilities for a renaissance in making scientific biology, as contrasted with journalistic biology, a dynamic force. If we could hybridize the material in *Science* and the English periodical *Nature,* to afford a weekly exposition of things biological, it should prove a success. I trust that the new venture will not depart too widely from sound biology. I also hope that the section on genetics and evolution will be retained at least in its present size. There is a desperate need for avenues of publication in these two fields. They should be expanded rather than contracted. My own positive suggestion for the fields of biology and genetics would be to assure a proper emphasis on the applied fields of biology and genetics. A very lively development of these biological sciences is emerging. You should make certain that these phases of biology have proper editorial representation and sympathy. With a sincere wish that the new THE AMERICAN NATURALIST may be born with a healthy genotype and with a resiliency to withstand the shocks of a changing environment.

E. W. LINDSTROM

IOWA STATE COLLEGE

Your letter of December 11 requesting me to write expressing good wishes for the new THE AMERICAN NATURALIST received. I certainly wish the new publication the best of luck, but I wonder whether the wires have not gotten crossed somewhere. I have never subscribed to THE AMERICAN NATURALIST; I am an anthropologist, not a biologist.

RALPH LINTON

COLUMBIA UNIVERSITY

I appreciate your letter of December 11 and I am interested to know that you are planning to bring out the new type of journal in THE AMERICAN NATURALIST. Such a journal as you describe should have a wide field of usefulness. I shall look forward to seeing the new publication. If possible, I shall do something to contribute to its success.

E. V. MCCOLLUM
THE JOHNS HOPKINS UNIVERSITY

I am delighted to learn of the new THE AMERICAN NATURALIST, which is to be representative of all biologists. Many congratulations to you and the best of wishes for the success of the change in THE AMERICAN NATURALIST.

GEORGE R. MINOT
THORNDIKE MEMORIAL LABORATORY,
BOSTON

Your plans for THE AMERICAN NATURALIST seem to me to promise a restoration of it to its original purposes. When I was first a contributor to its pages, about 50 years ago, it had long been publishing such things as you propose: "News of biologists and news of biological interest." Though I have been a regular subscriber, I have not been reading much of its contents for some years, because I do not know the technical language in which they are so largely written. I wish you great success in your effort to make it a magazine for naturalists again.

JAMES G. NEEDHAM
CORNELL UNIVERSITY

I thank you for your letter of December 11 in which you tell me about your plans for THE AMERICAN NATURALIST. I welcome your invitation to become a contributor.

R. M. OGDEN
CORNELL UNIVERSITY

This is to wish you well in your venture with THE AMERICAN NATURALIST.

WALTER W. PALMER
PRESBYTERIAN HOSPITAL,
NEW YORK CITY

I heartily agree with your plan to make THE AMERICAN NATURALIST a more representative journal for biologists by increasing its issues and developing its substance. The plan has my most sincere good wishes.

G. H. PARKER
HARVARD UNIVERSITY

I am deeply interested in your plans for publishing THE AMERICAN NATURALIST. The weekly issue feature is certain to add to the value of the regular monthly issue, and will be much appreciated by all biologists. May I take this opportunity to extend congratulations and to wish you continued success.

J. T. PATTERSON
UNIVERSITY OF TEXAS

I am greatly interested in your statement of the new plans for THE AMERICAN NATURALIST. I am glad to note that you are planning to introduce a weekly issue of more or less popular value and with news items of interest to biologists. It has seemed to me for some time that the field of biology was not at present adequately represented in the journals or other periodicals in comparison to the interests of physical science. I see no reason why your venture should not receive the unanimous approval of biologists and that it will be completely successful.

R. J. POOL
THE UNIVERSITY OF NEBRASKA

Please accept my best wishes for the deserved success of THE AMERICAN NATURALIST in its new form.

W. T. PORTER
THE HARVARD APPARATUS COMPANY,
DOVER, MASS.

I am much interested in your plans in regard to THE AMERICAN NATURALIST. I was always sorry it ceased to be a journal of Natural History as it was in the old days.

A. M. REESE
WEST VIRGINIA UNIVERSITY

I have your very informative letter of December 11th, regarding the change planned in THE AMERICAN NATURALIST. I have always regretted that this almost classic publication ever departed from the broad field it originally occupied and became so largely a cytogenetic journal. My regret I found shared by many others, to whom the "Naturalist" as it appeared in the days of Cope was a concrete expression of what a journal with that name should be. The name naturalist to us still has its old-fashioned flavor and meaning, and the pages of its issues of the eighties and the nineties helped to develop the interests which produced many scholars now elderly or who have already gone to their reward. To one to whom THE AMERICAN NATURAL-

IST as he knew it in the nineties was an inspiration and an incentive, I should most cordially welcome the proposed change. Old-fashioned enough to be a naturalist in the classic sense of the word, I feel there is a great need for more journals sufficiently authoritative, and yet not restrictedly technical, to inform the biologists of many phases and angles of a great field, and at the same time to be readable enough to develop needed recruits for both the needed groups of investigators, and the equally important army of supporters, to which all institutions and laboratories not state or foundation supported must turn continually for assistance. The constantly reiterated charge made against scientists is that they do not present their sciences in sufficiently understandable form for the intelligent and interested layman to follow them. In many cases the indictment is justified, and our chief defence is that we lack media for such type of expression. When we endeavor to meet the challenge we are recognizing our duty to our field of work and to those who support, directly or indirectly, that which we do. No one appreciates this more fully than an administrative officer of a research institution which must draw its support from the interested layman. Every means for meeting this criticism should be welcomed and utilized. I believe and sincerely trust that THE AMERICAN NATURALIST can be a valued means toward that end.

JAMES A. G. REHN,
Corresponding Secretary and Curator of Insects
THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

I am interested in the information contained in your letter to me of December 11, in which you outline the change in character which you propose to bring about in THE AMERICAN NATURALIST. I am sure that my hope for the success of your effort is shared by all the biological investigators of this country.

A. N. RICHARDS
UNIVERSITY OF PENNSYLVANIA

Any extension of the services earlier rendered by a biological journal should be welcome. When an extension of both publication and news service is provided by a journal of national and international scope the event should be doubly welcome. The end of World War II leaves us with fragmentary knowledge or with no knowledge, of our colleagues in most parts of the world. Perhaps my own favorable reaction to this move by the new AMERICAN NATURALIST is influenced by the circumstance that I have spent the past three months in South America. Here it is less easy than in the United States to obtain news and information of biological interest; such information, however, is as much desired by biologists here as it is elsewhere.

OSCAR RIDDLE
MONTEVIDEO, URUGUAY

I am much interested to learn of your plans for THE AMERICAN NATURALIST. I believe there is a real need for a weekly periodical which would bring together items of interest, thoughts and observations on widely diverse aspects of biology. Furthermore, I feel that such a medium would be of definite cultural value. Very best wishes to the new AMERICAN NATURALIST for a propitious birth and a successful career.

O. H. ROBERTSON
THE UNIVERSITY OF CHICAGO

May I offer my best wishes to THE AMERICAN NATURALIST in the initiation of a new chapter in the history of an old and valued friend. I expect I may have a short contribution of the sort you mention to send on in a few weeks.

ALFRED S. ROMER
HARVARD UNIVERSITY

I am glad to know that THE AMERICAN NATURALIST is expanding its scope and usefulness. It has my blessings.

M. J. ROSENAU
THE UNIVERSITY OF NORTH CAROLINA

I have your letter of December 11 in regard to the proposed change in THE AMERICAN NATURALIST. You may be sure that I will cooperate in every possible way to make the new magazine successful. May I ask, however, if in getting out a "weekly issue containing eight pages of news of biologists and news of biological interest" you are not overlapping the field of *Science*. I would not know from your description just what materials would go to *Science* and what items would be more appropriate to THE AMERICAN NATURALIST. I have no doubt that you are clear on this, but it would be helpful to me and to my associates if we could have a few more details about the nature of the magazine.

ALEXANDER G. RUTHVEN
UNIVERSITY OF MICHIGAN

I am interested in your proposed reorganization of THE AMERICAN NATURALIST. There is a need for greater union of all biological sciences and a need for a publication which will promote integration of all fields in biology. I shall be glad to continue to serve on the editorial committee.

KARL SAX

HARVARD UNIVERSITY

I am delighted to hear of the change in plan of THE AMERICAN NATURALIST. I like, too, the planned policy of avoiding highly technical articles. Certain of our journals are restricted to quantitative research. If you have recorded numbers and drawn curves your article is accepted. No thought or interpretation is required. Constructive speculation is frowned upon, and thinking beyond your experiment taboo. We have been characterized by our European colleagues as uneducated specialists. Industrialization and the narrow policy of our journals have made us so. I am, therefore, delighted that you are going to let us talk about our work in the new THE AMERICAN NATURALIST. May I also express the hope that the new journal will not be in control of any small group, clique or society, but will be truly American, its pages open to all scientists alike. But I see you have anticipated this, for you say, the NATURALIST is to be representative of all biologists. Just one criticism: I should much prefer having each issue alike in value, size and interest. An issue is much more likely to be read if news is combined with science. All good luck to you and the new journal, whatever its policy, so long as it is a broad, liberal and impartial policy in which research is recognized as involving both fact-finding and correlation through interpretation.

WILLIAM SEIFRIZ

UNIVERSITY OF PENNSYLVANIA

I commend your proposed changes in THE AMERICAN NATURALIST as being of great assistance in cooperative research projects, in interpreting allied technical contributions, and as valuable source material for the future historians of science.

W. M. SMALLWOOD

SYRACUSE UNIVERSITY

I would be glad to tender you all good wishes for THE AMERICAN NATURALIST, but I would have to confess that I have not been reading it. My time is so taken up that I find that I can not do all that I would like, and this is one of the things that I felt I must do without! However I do wish you success.

TORALD SOLLMANN

WESTERN RESERVE UNIVERSITY

I was very much interested to receive your letter of December 11th telling of your plans for the new THE AMERICAN NATURALIST. This new journal will fill a gap and will be of great service to all biologists. I wish it all success and shall follow its contents with the greatest of interest. At present, I do not have any contributions for it, but when I have such I shall keep you in mind.

G. LEDYARD STEBBINS, JR.

UNIVERSITY OF CALIFORNIA,
BERKELEY

Thank you for your letter in which you inform me of the planned changes in THE AMERICAN NATURALIST. While I wish you personally all success in your new venture, I am not sure of the desirability of the change contemplated. It seems to me that *Science* has published sufficient news regarding biologists and other scientists and I have not infrequently felt that some of the news was rather of a gossipy, and therefore not relatively important, nature. I am afraid of having eight pages every week devoted to biologists and biology topics alone. I shall be glad to be convinced that my present feelings are unjustified, but you will permit me an attitude of hesitation.

CURT STERN

UNIVERSITY OF ROCHESTER

I have your letter concerning changes planned for THE AMERICAN NATURALIST. The arrangement seems to me ill advised. THE AMERICAN NATURALIST is one of the oldest American scientific journals and has a distinguished tradition. As a bi-monthly organ devoted to the advancement of the biological sciences, with special reference to the factors of evolution, it has a distinct place and function. Furthermore, there is, I believe, an understanding with the American Society of Naturalists which undoubtedly is beneficial to both parties. Without the previous approval of the society, it is possible that the contemplated change might impair this mutual understanding. As I am sure you are well aware, there is a critical group in the Naturalists that might seize upon the proposed

change to renew the former controversy. I see no necessity for an additional journal to publish the kind of articles mentioned in your letter and so far as the weekly issue is concerned, it appears again to be an unnecessary duplication of *Science*. Presumably you have already considered the points raised, but I wish to emphasize them.

HORACE W. STUNKARD
NEW YORK UNIVERSITY

I have your letter about THE AMERICAN NATURALIST. As one well past the retiring line, I am progressively a spectator rather than a writer of technical papers. As to publications of scientific material in general, I hope that there will be a progressive segregation—(1) the separation into clearly marked groups in which reports will be strictly technical for the technical, (2) a second general section in which one with a fundamental interest and a general knowledge of scientific matters can open at random and read without mystification. Even in a technical journal, I want an author to place his subject-matter in relation to something a reasonable percentage of his casual scientist readers will recognize. One ought to be told whether the "varmint" discussed or described is related to the "owl" or the "pussy-cat." So if you can get a stream of papers which will give the ordinarily interested reader a sound story in terms he can understand—so arranged as to leave a perspective setting in one's mind, your paper will be worth while. Go to it.

CHARLES THOM

I am interested and pleased by your plan to make THE AMERICAN NATURALIST a weekly representative of all work in biology. I wish you the best of fortune in this new enterprise.

E. L. THORNDIKE
NEW YORK, N. Y.

In reply to your letter of November twenty-seventh, in which you outline plans for making THE AMERICAN NATURALIST a weekly, without abandoning a monthly number with the functions that now exist, let me add my word of good wishes and hope for what the new project may do to further the interests of workers in the biological sciences. There is no evolution without differentiation. With best regards, I am

HARRY BEAL TORREY
BERKELEY, CALIF.

I was interested to learn from your letter of December 11 that you intend to make THE AMERICAN NATURALIST a weekly publication. It should prove valuable and useful to all biologists to have a biological journal of frequent issue, containing general articles as well as the more technical ones on genetics and evolution in which practically all biologists are interested and, in addition, news items, current events, book reviews, etc. With best wishes for success of the new THE AMERICAN NATURALIST,

ALBERT TYLER
CALIFORNIA INSTITUTE OF TECHNOLOGY

I was very much interested to hear about the proposed changes in the complexion of THE AMERICAN NATURALIST, as indicated in your letter of November 27. I am in favor of any move that would strengthen and promote the interests of biology as a whole, and I am sure that you are aiming in the same direction. I hope that THE AMERICAN NATURALIST will continue to serve its purpose as an organ of integration among biologists. With best wishes to that effect,

PAUL WEISS
THE UNIVERSITY OF CHICAGO

I am not sure that I approve of this attempt on your part to set up a publication to compete with *Science*, which is being taken over by the A. A. A. S. I am willing to subscribe for a year to see how the new journal works out, provided a satisfactory adjustment is made in regard to my previous subscription.

D. H. WENRICH
UNIVERSITY OF PENNSYLVANIA

I sent to you the other day my check for subscription to the new THE AMERICAN NATURALIST. I think all the friends of this journal will be interested in its prospects for its new appearance. All of us are interested in news of various biologists of the country and world and if this can give us more news than *Science* we shall be quite pleased. Many scientific journals have strictly scientific articles, but the public really needs one that is semi-scientific and of popular interest. I think that your plans for THE AMERICAN NATURALIST may fill this need, and render it more popular to the ordinary layman. Many readers become discouraged with these technical papers, and due to special vocabularies in the paper they are unable to

understand its content. We shall await the publication of THE AMERICAN NATURALIST with a good deal of interest and later may be able to give some constructive criticism, but we hope it will be so good that it can not be improved. With all good wishes in this new venture,

DAVID D. WHITNEY
UNIVERSITY OF NEBRASKA

Thank you very much for your letter informing me of the changes you have planned for THE AMERICAN NATURALIST. There is without question a need for a weekly publication designed to keep biologists in general abreast of the current work in this vast field. Wishing you every success in this desirable objective,

M. C. WINTERNITZ
YALE UNIVERSITY SCHOOL OF MEDICINE

I was greatly interested to learn of your plans of expansion of THE AMERICAN NATURALIST. They promise to increase the usefulness of this journal, which already enjoys a preferred standing among biological periodicals of its class. With good wishes,

EMIL WITSCHI
STATE UNIVERSITY OF IOWA

I have always felt that THE AMERICAN NATURALIST was a useful publication, but at the present juncture I agree with you that its value probably can be greatly increased by certain changes in policy. My own hope and wish would be that it should represent more adequately than it has recently the fundamental interests and the activities of the field naturalist. We have an adequate supply of popular naturalistic journals, but we lack in this country an organ for those strictly scientific studies of animal and plant life and behavior which are somewhat too technical for the lay reader and would not naturally find place in the journals which provide for experimental studies. I am sure that you realize there is an intermediate field between the experimental and the amateurishly naturalistic type of observation which deserves more adequate publication provision. I am indeed delighted by the constructive effort you are making to increase the value of THE AMERICAN NATURALIST and I earnestly hope that its new form may appeal widely to those who are interested in animals and plants and their varied environmental and other relations.

ROBERT M. YERKES
YALE UNIVERSITY SCHOOL OF MEDICINE

RECENT DEVELOPMENTS IN SCIENCE LEGISLATION

By Professor ROBERT CHAMBERS
Department of Biology, New York University

There has been considerable airing in *Science* and in the press, also in statements distributed to various scientists throughout the country as to the relative merits of the proposals being made by Senator Kilgore (S.1297)[1] and by Senator Magnuson (S.1285) for the formation of the federally sponsored National Science Foundation.

A presentation of the merits of the Magnuson Bill was given in the Letter to the President which appeared in the November 30 issue of *Science*. A recent report of the hearings on Science Legislation from October 8 to November 2 is contained in an Analytical Summary of Testimony presented (or given) at the hearings. The most recent, December 21, is the preliminary report of a completely new subcommittee draft of Senator Kilgore's bill. These reports can be obtained by writing to the Committee on Military Affairs, Senate Building, Washington, D. C. The new draft is a thorough revision of the original bill with an elimination of the most objectionable features and the inclusion of many constructive measures.

The fact that the formal organization of the Foundation is of great concern to the scientists of the country, is recognized by a statement in the analysis printed with the new draft that the sub-committee "looks forward to a further discussion of this problem before reporting out a final bill to the Senate Military Affairs' Committee." The present draft, as in the original bill, calls for a full time administrator to be appointed by the President and confirmed by the Senate. This form of appointment introduces a feature, hitherto lacking, of having science represented in our government practically on a par with other government departments. The President is also to appoint a scientific board consisting of nine members "from among persons who are specially qualified to promote the broad objectives of this act" plus the chairmen of the several divisional scientific committees. Except for the Division of National Defense which will be related to the war and navy departments, the personnel of the divisional committees will be appointed by the administrator "solely on the basis of their demonstrated capacity to carry out the purposes of the Foundation. . . ." The divisions constituting the basic sciences are to be a Division of Mathematical and Physical Sciences, a Division of Biological Sciences and a Division of Social Sciences. The others are a Division of Health and Medical Sciences, a Division of Engineering and Technology, a Division of Scientific Personnel and Education, a Division of Publication and Information. There are also to be facilities for additional divisions to cover possible future developments. To insure fair distribution of funds, percentage figures are given for apportionment among the states of the Union to tax-supported colleges and universities, also for private non-profit organizations.

Concerning the Division of Publication and Information a feature brought up in the analysis of the draft is that publications and assistance be given to accredited non-profit scientific journals. This might well be included with no provisional conditions in the body of a future draft. Three other features are: the awarding of scholarships and fellowships for scientific study and research, the maintenance of a central clearinghouse for information with a register of scientific and technical personnel, and provision for international scientific cooperation with the approval of and through the State Department. Included will be the defraying of expenses of repre-

[1] *Science*, 102: 2661, 630, 1945.

sentatives to international scientific meetings.

Concerning the top administration of the Foundation there has been considerable questioning as to the danger of undue political involvement if there be an over-all Administrator who is not specifically appointed by scientists and who shall only consult and advise with his Board. Those of us who approved the principles embodied in the Magnuson bill had this primarily in mind. However, accepted Congressional procedure is claimed to require a full-time governmental administrator. Therefore, in order to circumvent arbitrary action on the part of the Administrator counter to the desire of working scientists, the new draft has been formulated with the following significant features: (1) inclusion of specific language showing the intent of Congress that scientific freedom and initiative be safeguarded; (2) provision for a "goldfish bowl" type of operation, so that the work of the Foundation will be under the constant scrutiny of the public and particularly of the scientists; (3) provision for the part-time Scientific Board and Committees to have direct access to the President, Congress and the public not only for guidance but also as a check on the Administrator. One of the strongest points concerning this is (Section 4d) that the Board and the Divisional Committees will not only have full access to all information in possession of the Foundation except such as may be classified for reasons of national security but also that the annual report of the Administrator shall include whatever dissenting opinions may be submitted by individual members of the Board or the Divisional Committees.

The hearings in Washington during October have gone far toward crystallizing the desires of scientists with reference to Science Legislation. The biologists have demonstrated clearly to the Senators that the biological sciences merit a position on a par with the other basic sciences. The publicizing of the Letter to the President by the Bowman Committee has brought forcibly to the front the keen conviction of the scientists that their research must be untrammeled by any political consideration, and that scientific freedom and initiative must be fully safeguarded. It now remains to be worked out which is the best way to realize these objectives in the constitution of a National Science Foundation.

We all want a nationally sponsored foundation and now is the psychological time to secure it. America possesses the facilities and should take over the prerogative which for so long had been assumed by Germany. This can be realized by a national organization in which the basic sciences are given a preëminent position.

We scientists should be willing, as citizens, to work within a constitutional governmental framework as long as we are assured of freedom from restraint by arbitrary authority be it either of a single administrator or a board of individuals.

THE FUTURE OF ENTOMOLOGY

By Professor CHARLES T. BRUES
Biological Laboratories, Harvard University

PREDICTIONS of things to come commonly appear quite plausible at the time of their presentation, and fortunately they are too soon forgotten to mete out just punishment to their perpetrators. Economists are particularly prone to extrapolate the trends of the business world, but as they never agree, always some one is right and ready to continue the game. Biologists, it may be said to their credit, rarely go the way of the seer. Nevertheless, it is evident that predictions of future progress based on present activities may be made with greater assurance at some times than at others. Such appears to be the case with entomology at the present time.

During the past few decades insects have furnished the material on which a very considerable part of our present knowledge of heredity is based. This was a wholly unexpected turn of events, following the chance discovery of a mutation in the fruit-fly, Drosophila. It so happened that this insect was admirably suited for the study of unit characters on account of several characteristics not known nor appreciated at the time the fly first attracted attention. Such developments are wholly unpredictable: they are purely coincidental, and they can be expected to recur only at long intervals. In spite of the immense amount of work that has already been done on Drosophila it is evident that the end is not yet. Mutations can now be produced on order, although still with random results. We can not say how far such procedures may be extended or applied to other animals, and one might indulge in quite fantastic speculations as to what the future may have in store. Some of my colleagues will insist that this is genetics, not entomology. This is a fine point, as some geneticists will admit an acquaintance with entomology and *vice versa*. Beyond this, the production of mutations may even be extended far into the realm of nuclear radiation and become of direct interest to the physicist.

During the war almost unbelievably rapid progress was made in the several sciences with relation to problems arising in connection with military activities. Unfortunately the designation of "secret" has been applied to much that was learned at that time, and a swarm of befuddled politicians are now busily engaged in plotting the future progress of science in anticipation of future wars. They are thus further complicating a wholly unpredictable concatenation of events by the enactment of laws relative thereto.

As much of the war was fought in regions blessed with innumerable pestiferous insects, and many which spread serious human diseases such as malaria, bubonic plague, filariasis, leishmaniasis, yellow fever, typhus and still others, there has been great activity in the study of such insects in connection with the health of the armed forces. This has been mainly of a technological nature leading to a better control of the insect vectors. Highly important discoveries of new synthetic insecticides such as DDT, and repellents, such as certain thallates for protection against the bites of mosquitoes, mites, ticks and various epizoic insect parasites, have opened up an entirely new field for the economic entomologist. It is now safe to predict that the near future will see practically a revolution in our methods of driving away and destroying noxious insects. Agriculture, forestry and public health, as well as household comfort, will undoubtedly share

the benefits. It is reasonable to expect also that still more efficacious substances will be found. Already the arsenicals and poisonous alkaloids, like nicotine and rotenone, and repellents, like tar and citronella, are in the discard.

In connection with the application of these new substances much basic physiological work has been done and we can look forward to extensive additions to our knowledge of insect physiology, especially with reference to toxicology. Since experimental work of this sort on insects, oxygen poisoning, for example, often leads to a better understanding of similar problems in mammalian and human physiology, such matters are bound to receive attention at the hands of entomologists to the benefit of other branches of biology and in medical research as well.

The standardized behavior of insects, not only in their instinctive processes, but in the simpler types of muscular and neural activity, make them ideal objects for the experimentalist in connection with such phenomena as drug action, fatigue, stimulation, senescence and a host of others of great variety. The availability of pure-bred lines of several kinds of insects is a further incentive to work of this sort.

The intense vitality of insects makes it possible to study their growth, development and regenerative processes not only in the decerebrated condition, but even after decapitation, transsection or worse. They thus offer opportunities to apply to highly organized animals the technique lately in vogue with flatworms and the like. We may expect interesting and important discoveries relative to cellular and tissue physiology to result from such studies, provided, of course, that the antivivisectionists do not take the insects under their wing. This seems hardly probable as they have so far neglected tissue culture *in vitro*, incidentally something which may be readily practised with insects.

During the past several decades considerable attention has been paid to the study of "races" or "strains" of insects, probably representing incipient species. Such categories do not represent a new idea, as evolutionary speculation since the time of Darwin has postulated the necessary existence of something of this sort. Several clear-cut cases have been demonstrated among vegetarian insects. More recently, following earlier and somewhat vague observations, it has been shown that the malarial mosquitoes of Europe (*Anopheles maculipennis*), previously regarded as a single form, are segregated into several types that differ in host preferences and other physiological characteristics, although they are separable on morphological grounds only by the color pattern of the eggs. Similar conditions prevail among the malarial mosquitoes in other parts of the world, and it is obvious that these peculiarities, yet to be fully elucidated, are of immense importance to public health, and that they will be the object of extensive future studies.

Such discoveries are very disconcerting to the insect taxonomist, already harassed by the seemingly endless variety of the entomological fauna. They dread the eventuality that insect classification may be thrown into the disorder that prevails within the precincts of the bacteriologists and virologists who are already hopelessly adrift in a conglomeration of strains, mutations and types among human pathogens.

The quite recent demonstration of hormones in insects has opened up a new line of investigation to entomologists interested in the phenomena of growth and metamorphosis. Thus some of the age-old questions relating to the development of the higher insects may be answered, or at least better understood in the near future.

Some of the most interesting technological problems facing the entomologist arise through the artificial spread of destructive insects into new regions. Such dissemination has been going on at a rate accelerated in direct proportion to the increased ease of travel and shipment of materials by commercial agencies. The phenomenal growth of air travel has suddenly increased this

hazard, with the promise of untold trouble in the future. Occasionally, the application of vigorous measures has eliminated introduced species, for example, the gipsy-moth in New Jersey, the Mediterranean fruit-fly in Florida, the African malarial mosquito (*Anopheles gambiae*) in Brazil, and the Colorado potato-beetle in England, but such cases are statistically negligible among the pests that have been spread far and wide, to remain and multiply until eternity so far as we can foresee. Much of our future comfort and prosperity depends upon how satisfactorily we may be able to meet this challenge.

Another technological phase of entomology is the utilization of the insect pests of weeds and undesirable plants to reduce or exterminate such offending forms of plant life. The reclaiming of vast areas of Australian agricultural land that had been over-run by American prickly-pear cactus is an example of what may be done in this way to mitigate the damage caused by alien weeds. We may expect a considerable extension of this method, whose application, it must be remembered, is, like playing with fire, fraught with danger when undertaken without a full understanding of the food habits of the insects involved. Let us hope that the water hyacinth, for example, may fall prey to some insect pest.

During the past fifty years, which quite exactly coincides with your prophet's interest in insects, the greatest problem cast into the lap of the entomologist has been the unravelling of the part played by the insects and other arthropods in the spread of infectious diseases of man. This job has been valiantly undertaken, but it is still unfinished, and the end is not yet in sight. It is hard to believe that any equally important and spectacular entomological field still lies completely hidden and unsuspected, but it would be unwise to discount such a possibility.

THOMAS HUNT MORGAN

See Photograph on Front Cover

THOMAS HUNT MORGAN was born at Lexington, Kentucky, on September 25, 1866, and died at Pasadena, California, on December 4, 1945. He received the B.S. degree from the University of Kentucky in 1886 and the Ph.D. from Johns Hopkins University in 1890. From 1891 to 1904 he was professor of biology at Bryn Mawr; from 1904 to 1928 professor of experimental zoology at Columbia University, and from 1928 to 1945 professor of biology at the California Institute of Technology.

At Johns Hopkins he was greatly influenced by Brooks, Martin and Howell. In his earlier years he made several stays at Naples, where he formed close and lasting friendships with Driesch and Dohrn. His long association with Wilson at Columbia was an outstanding example of mutual influence and helpful stimulation; those of us who had the privilege of studying there at the time can appreciate but can scarcely describe adequately the spirit of the laboratory under these two. For many years Morgan spent his summers at Woods Hole, and there he formed close friendships with many of his colleagues. Here again the atmosphere of the laboratory can only be fully appreciated by those who experienced it.

Morgan's early work was in embryology, beginning with descriptive accounts of the frog, Balanoglossus, and the Pycnogonids. His interest early shifted to the experimental attack on embryological problems, and he soon became a leader in the rising school of experimental embryologists. This work led naturally into the field of regeneration, with which he was occupied for many years.

Soon after the rediscovery of Mendelism, Morgan began his genetic work, at first with mice and rats, and about 1909 with Drosophila. He was also at this time interested in the life-cycle and the determination of sex in Aphids and Phylloxerans. His work on these forms was of great importance in the history of genetics; for he showed that a complex situation, that seemed at first sight to be wholly incompatible with the chromosome theory of sex-determination, was in fact fully in accord with that theory and was only comprehensible in its terms.

It is his work with Drosophila that is best known. The first important result here was the discovery of sex-linkage of the type to be expected from the cytological evidence then available. This was quickly followed by the demonstration of exchange between homologous chromosomes, and of linkage between sex-linked genes. These discoveries laid the groundwork for the modern chromosome theory of heredity; and a large part of the structure that has been reared upon them was due to the work of Morgan and his students. As one of those students, I can testify to his unfailing support, stimulus and protection. He built up a group of workers, and saw to it that they were left free to carry on their work independently yet with a remarkable spirit of cooperation derived from him.

Morgan's title at Columbia was that of professor of experimental zoology; and it was an appropriate title, for he always emphasized the experimental approach and utilized it in many fields. He was also a good naturalist; it was not easy to present him with an animal that he could not name. In addition, he was remarkably skilful at keeping animals alive and in good condition in the laboratory.

He was essentially a pioneer in his scientific work. He was at his best in opening up new and little understood fields. He appreciated the value of detailed and analytical studies in better known fields, and was always ready to support such work, but he himself was usually off on some new and less highly cultivated line. He always had

many types of work under way—a fact that is only partly shown by the diversity of subjects on which he published, for many of his projects did not yield significant results, and were never recorded. He was always inclined to use simple techniques and little elaborate equipment, and it was an effort for him to appreciate the necessity for expensive apparatus in so much of modern experimental biology.

By those who knew him well he will always be remembered for his quick wit, sympathetic understanding and unfailing generosity. His students affectionately referred to him as "The Boss"—a term intended to imply their respect for him as a scientist and as a man.

Many honors were conferred upon him; it is perhaps sufficient to record that he was president of the National Academy of Sciences (1927–1931), president of the American Association for the Advancement of Science (1930) and Nobel laureate (1933).

A. H. STURTEVANT

JOHN CAMPBELL MERRIAM

THE death of John Campbell Merriam at the age of seventy-six years on October 30, until his retirement as emeritus president in 1939 for eighteen years president of the Carnegie Institution of Washington, recalls his long and devoted service to science in the United States. Prior to his work at the Carnegie Institution he had been a member of the faculty of the University of California, which he joined as instructor in 1894, becoming professor of paleontology in 1912 and dean of the faculties in 1920. In that year he resigned from his professorship to become head of the Carnegie Institution in Washington, D. C.

Dr. Merriam served as chairman of the National Research Council in 1919; since 1928 he was regent of the Smithsonian Institution; vice-president of the National Academy of Sciences; president of the Pacific Division of the American Association for the Advancement of Science, 1919–20; president of the Geological Society of America, 1910; president of the American Paleontological Society, 1917; member of the California Academy of Sciences, San Francisco, and many other scientific organizations. He was president of the executive committee of the Pan American Institute of Geography and History, 1935–38; chairman of the research committee of the California State Council of Defense, 1917–20, and was for several years honorary president of the California Conservation Council.

Honorary degrees were conferred on Dr. Merriam by Columbia, Princeton, Yale, Pennsylvania, Oregon State College, California, New York, Michigan, Harvard, George Washington, the University of Oregon, and others.

Dr. Merriam retired in 1937 from the presidency of the Carnegie Institution of Washington at his own request in order that he might be free to devote his time to research and to writing. The board of trustees, on December 10, 1937, appointed him president emeritus with provision for support of his studies. Quoting from the foreword to the volume entitled "Published Papers and Addresses of John Campbell Merriam":

"The trustees arranged for the assembling and reprinting of his papers, monographs, reports, lectures and addresses. In this series are gathered together his many publications grouped according to general subject, together with a bibliography arranged chronologically. There will be found in these volumes a record of research in paleontology, with clear evidence of a well-planned investigative program and of superb achievement in the comparative anatomical, particularly vertebrate, aspects of this branch of biological thought.

Photograph by Bachrach

JOHN CAMPBELL MERRIAM

"Shortly after this action a small group of the members of the institution suggested that a dinner be given in his honor and that a testimonial volume of papers written by members and research associates of the institution be presented to him at that time in appreciation of his far-sighted leadership. The trustees approved the proposal and appointed a small committee to cooperate in the project. After thorough discussion, the committee concluded that the volume should comprise papers on current problems, conducted either entirely or partly within the institution, and illustrative of the principles followed by Dr. Merriam during the eighteen years of his presidency of the institution."

These volumes were issued in 1930. They were entitled "Cooperation in Research" by staff members and research associates of the Carnegie Institution of Washington.

Quoting again from the foreword is the following appreciation of Dr. Merriam's work:

"The trustees of the Carnegie Institution of Washington are glad to acknowledge their debt of gratitude to Dr. John Campbell Merriam, who has served them so effectively as president of the institution these many years. Coming to the institution from an important post in a great university, Dr. Merriam brought administrative experience and understanding, outstanding charity and kindness in his relations to staff members and employees alike, amazing appreciation of the possibilities for advance in knowledge in the many fields of science and the humanities, and extraordinary abilities and wisdom in the choice of men and projects. A rare investigator, a thoughtful and forceful philosopher in science, a sagacious organizer of successful cooperative undertakings in research, an ardent expositor of the value of original investigation to the general public, a wise leader—Dr. Merriam has made notable contribution to human progress and well-being; the trustees of the institution feel that these volumes of Dr. Merriam's publications and addresses witness this contribution."

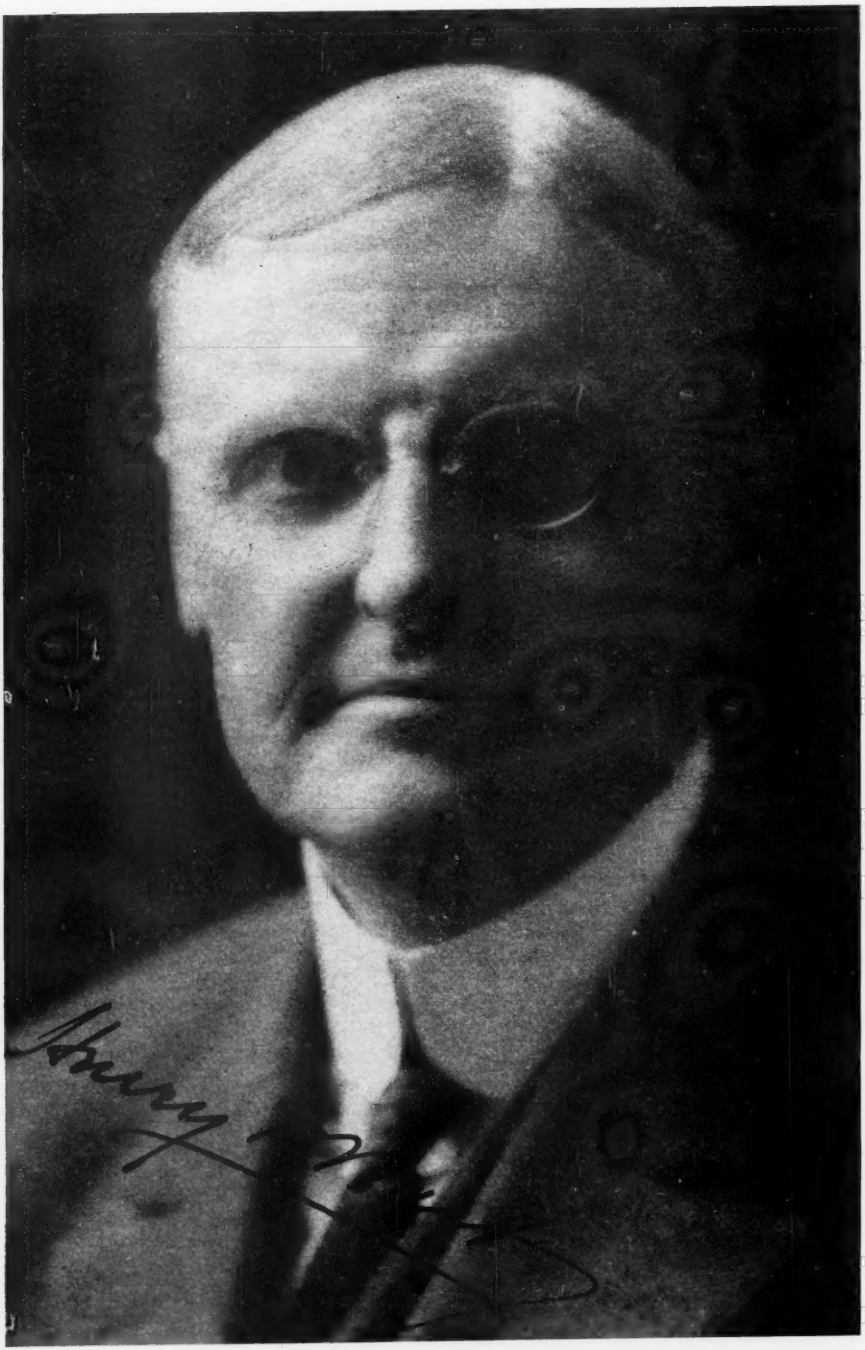
HENRY BALDWIN WARD

THE death on November 30 of Dr. Henry B. Ward, professor of zoology at the University of Illinois, at the ripe age of eighty years removes an outstanding figure among those who have devoted their lives to the advancement of science. In this work he was actively engaged until the end.

Dr. Ward retired with the title emeritus from the professorship at the University of Illinois in 1933, but this did not mean the relinquishment of the work in which he was so deeply interested. Indeed in the years that followed his accomplishment in his chosen field seldom, perhaps never, has been surpassed. His influence was very great. His tall, massive figure, with slightly stooping shoulders from which his head bent forward, together with the eager expression of his face and evident interest in everything going on around him, made him a conspicuous figure in any assembly at which he was present.

Dr. Ward was born at Troy, N. Y., in 1865. His undergraduate work was done at Williams College, after which he was for two years instructor in a high school at Troy. He then entered Harvard University as a graduate student, receiving the degree of Ph.D. in 1892. He studied at Göttingen, Freiburg and Leipzig, at the last university as a student of Professor Rudolph Leuckart, eminent for his work in parasitology. From this time on Dr. Ward's research was devoted to study in this field.

For a year after his return to the United States, Dr. Ward was an instructor in zool-



ogy at the University of Michigan. Then from 1893 to 1909 he was successively associate professor, professor and head professor at the University of Nebraska, becoming dean of the School of Medicine in 1902. He joined the faculty of the University of Illinois as professor and head of the department in 1909, retiring with the title of emeritus in 1933.

Dr. Ward established *The Journal of Parasitology* in 1914, and was the editor of the first eighteen volumes. In the summers he carried out investigations for the U. S. Fish Commission, especially on the conservation of salmon, and took a part in a number of field expeditions to the Pacific Northwest and Alaska.

Dr. Ward was permanent secretary of the American Association for the Advancement of Science from 1933 to 1937 and was an active member of many scientific societies. He was president of the American Society of Zoologists in 1912; of the Society of Parasitologists in 1925; of the Nebraska Academy of Science and of the Illinois Academy of Science in 1919. He was also a corresponding and a foreign member of various foreign scientific societies.

His death is an irreparable loss to science, and to those who were privileged to be called his friends.

AWARD OF THE DANIEL GIRAUD ELLIOT MEDAL

Dr. Theodosius Dobzhansky, professor of zoology at Columbia University, has been awarded the Daniel Giraud Elliot Medal of the National Academy of Sciences. On the occasion of the presentation of this award Dr. Fred E. Wright, petrologist of the Geophysical Laboratory of the Carnegie Institution of Washington, read an address by Dr. William K. Gregory, professor of paleontology at Columbia, given below, in appreciation of his work.

Mr. President and Members of the Academy:

As has already been stated, the Daniel Giraud Elliot Medal Committee recommended to the academy that for the year 1941 that medal and award be given to the distinguished author of the book entitled "Genetics and the Origin of Species," second edition, 1941. The committee's choice was confirmed by vote of the academy at the spring meeting last April and it remains only to present the author of that work.

In the judgment not only of your committee but of many other scientists, the book itself already ranks among that classic series of which the first was Darwin's "Origin of Species." Here are set forth with remarkable clarity what may be called both cross-sections and perspective views of the intricate problems encountered by the geneticists in their very real discoveries of the mechanisms of speciation. For a decade or two after the rediscovery of Mendel's principle, the emphasis of genetic studies was almost necessarily laid upon laboratory studies of the hereditary mechanism. During this period the problems of evolution in the field and forest received less attention and students of evolution at times tended to be divided into almost hostile camps; the taxonomists, collectors and museum men in one camp, the geneticists in the other.

The healing of this indefensible schism has long been under way and the last traces of it should be obliterated by Professor Dobzhansky's work. His chapters on Organic Diversity, on Mutation as a Basis for Racial and Specific Differences, on Variation in Natural Populations, on Selection, on Isolating Mechanisms, on Patterns of Evolution and on Species as Natural Units will be of unceasing aid to students of evolution in the open; while those on Gene Mutation, Chromosomal Changes, Polyploidy and Hybrid Sterility expose the

Photograph by Alman Co.

TH. DOBZHANSKY

mechanisms of evolution as they are revealed under the microscope and by statistical analysis.

Dr. A. H. Sturtevant, another eminent geneticist and taxonomist, has kindly sent me the following note: "The book of Mr. Dobzhansky summarizes the advances in the application of the results of modern genetics to the theory of evolution—in particular to the problem of species formation. It is of great importance, partly because the author is one of the very few men with an intimate first-hand knowledge both of modern experimental genetics and of the field of taxonomy. Another reason why the book—like the other contributions of Mr. Dobzhansky—has been of great influence is that it serves to make the mathematical developments of the theory more intelligible to working naturalists—who have been largely inhibited by the rigorous and formidable mathematical analyses of earlier authors."

Professor L. C. Dunn, in his "Editor's Preface" to the first edition of "Genetics and the Origin of Species," notes that "Professor Dobzhansky's book signalizes very clearly something which can only be called the back-to-nature movement. The methods learned in the laboratory are good enough now to be put to the test in the open and applied in that ultimate laboratory of biology, free nature itself. Throughout this book we are reminded that the problems of evolution are given not by academic discussion and speculation, but by the existence of the great variety of living animals and plants."

"In all this," Professor Dunn continues, "the author appears not only as geneticist and as student of natural history, but as one who received his training in both fields in Russia. English-speaking biologists have special cause to be grateful for this last fact, for it has enabled Professor Dobzhansky to make available to us many important contributions from workers in the Soviet Union, where researches in this field have been actively prosecuted."

Mr. President, I deem it an honor to present to you for the Daniel Giraud Elliot Medal and Award, Dr. Theodosius Dobzhansky, professor of zoology in Columbia University.

THE AWARD OF THE WILLIAM H. NICHOLS MEDAL

THE William H. Nichols Medal of the New York Section of the American Chemical Society has been awarded to Dr. Wendell M. Stanley, in recognition of his discoveries in the chemistry of viruses. Dr. Stanley is biochemist in the Department of Animal and Plant Pathology of the Rockefeller Institute for Medical Research at Princeton, N. J., with which he has been associated since 1931.

Dr. Stanley upset the prevailing belief as to the origin of disease in 1935, when he announced that he had isolated in crystalline form a disease-producing virus and had used it to reproduce the disease. By showing that infectious diseases are not necessarily caused only by microorganisms and their toxins, he opened a new field of investigation in the fight to provide immunity against diseases such as influenza, infantile paralysis, encephalitis and the common cold.

A study of tobacco mosaic disease led to the discovery. From the juice of infected plants five pounds of a crystalline protein was obtained which exhaustive tests indicated was the virus causing the disease. He showed that this virus could be transmitted from plant to plant, indicating multiplication or reproduction of the virus, and consequently that viruses, though chemicals without life, are able to reproduce themselves in the presence of living matter.

Since 1935 Dr. Stanley has developed chemical and physical ways of making viruses inactive while retaining their ability

DR. WENDELL M. STANLEY

to produce immunity. Last spring he announced a new influenza vaccine, ten times as effective as the strongest previously known.

In the citation accompanying the award, the American Chemical Society emphasizes his isolation of the tobacco mosaic virus as a turning point in the virus field. It reads:

"In 1935, Stanley isolated a crystallizable nucleoprotein of high molecular weight, which possessed the properties of tobacco-mosaic virus, from Turkish tobacco plants diseased with tobacco mosaic. For the first time it was possible to correlate virus activity with a definite, tangible, physical entity. During the past ten years, Stanley has amassed an array of data through exhaustive chemical, physical and biological tests which have indicated that this particular nucleoprotein is the tobacco-mosaic virus.

"Dr. Stanley not only made the signal discovery that was a turning point in the virus field, but in addition has created at Princeton a center for virus research. From his laboratory has come a series of brilliant contributions on the chemical and physical properties of various viruses, animal as well as plant."

Dr. Stanley was born in Ridgeville, Ind., on August 16, 1904, and received his bachelor of science degree from Earlham College, Richmond, Ind., in 1926. Continuing his studies at the University of Illinois, he received a master of science degree in 1927 and a doctor of philosophy degree in 1929. After teaching at Illinois for a year, he became an International Research Council fellow and studied for two years at Munich, joining the Rockefeller Institute on his return. Dr. Stanley was awarded a $1,000 prize in 1937 by the American Association for the Advancement of Science and the Isaac Adler Prize of $2,000 in 1938 by the Harvard Medical School. In the same year he received the Rosenberger Medal from the University of Chicago and the John Scott Medal, with a premium of $1,000 from the City of Philadelphia. The honorary degree of doctor of science was conferred on him by Harvard and Yale Universities and by Earlham College.

Dr. Stanley was the Sigma Xi national lecturer in 1938 and held the Hitchcock professorship at the University of California in 1940. He was visiting professor of chemistry at Earlham College in 1941,

THE WILLIAM H. NICHOLS MEDAL

and the following year he served as both Messenger lecturer at Cornell University and Vanuxem lecturer at Princeton University.

The Nichols Medal Award, which is conferred annually to stimulate original research in chemistry, was founded by the late Dr. William H. Nichols, a charter member of the American Chemical Society and chairman of the board of the Allied Chemical and Dye Corporation. The presentation to Dr. Stanley will be made on March 8, at a joint meeting at the Hotel Pennsylvania in New York of the New York Section of the Chemical Society and the Society of Chemical Industry.

THE DEAN OF THE COLLEGE OF VETERINARY MEDICINE OF THE UNIVERSITY OF ILLINOIS

PROFESSOR ROBERT GRAHAM has been named dean of the new College of Veterinary Medicine of the University of Illinois. He was the unanimous choice of the committee appointed to make recommendations for the new post.

Professor Graham has been a member of the University of Illinois faculty since 1917, and since 1941 has been head of the department of animal pathology and hygiene. He was professor of veterinary science at the University of Kentucky from 1911 to 1917. He is a native of Iowa, and received the degree of doctor of veterinary medicine from Iowa State College in 1910. He also received a degree from the University of Kentucky in 1912 where he served as veterinarian to the State Board of Health and as pathologist to the State Live Stock Sanitary Board.

During the first World War he was a captain in the army veterinary corps, and later a major in the reserves. In 1924–25 he was engaged in special investigations in animal diseases in Haiti. He developed the first anti-toxin for botulinus poison in man, at the University of Illinois in 1919, and in 1923, with I. B. Boughton, identified type-C botulism as the cause of limberneck in poultry.

He has had broad experience in veterinary medicine and the closely related fields of agriculture and public health.

He has made significant contributions to knowledge of forage poisoning, parasitism and of encephalitis in horses and mules; brucellosis, erysipelas, enteritis and so-called "baby-pig disease" of swine; parasitism, abortion and listerellosis of sheep; and the development of methods employed in sterilizing incubators used by commercial hatcheries and the preparation of egg-propagated vaccine used in immunizing chickens and turkeys against fowl pox.

Dr. Graham has done important work in the extension program in animal pathology, a part of the extension work of the College of Agriculture of the University and of the U. S. Department of Agriculture.

Since 1919 the diagnostic service of the Division of Livestock Industry of the Illinois State Department of Agriculture has been conducted in the Laboratory of Animal Pathology of the University under the direction of Dr. Graham. Evidence of its increasing service is shown in the record of 121,000 specimens submitted for laboratory diagnosis last year, as compared to 26,000 ten years ago.

In 1925 the U. S. Department of State in connection with the American occupation of Haiti asked Dean Graham to survey animal diseases in that nation. Animals were imported, instruction undertaken and clinics established under his direction.

He also has served the Federal Government as expert witness in giving court testimony exposing worthless remedies sold under false claims, saving livestock and poultry producers large sums of money. During World War II he was a member of the Illinois Veterinary Procurement and Assignment Committee.

APPOINTMENTS AT CORNELL UNIVERSITY

Two appointments at Cornell University have been made recently.

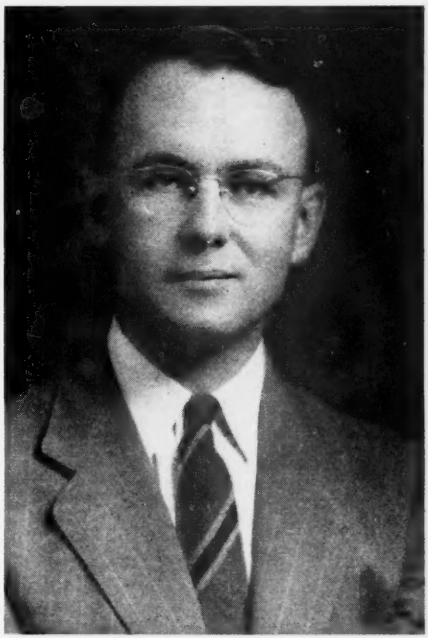
Dr. George C. Kent, of Iowa State College, who was recently appointed professor

DR. GEORGE C. KENT
PROFESSOR OF PLANT PATHOLOGY, CORNELL UNIVERSITY

of plant pathology at Cornell University, will take over the teaching of the general course in plant pathology, formerly given by the late H. H. Whetzel. He plans also to devote time to research. His natural preference would lead to further study of root diseases and certain cereal diseases.

At Iowa State College, Dr. Kent gave the general course and some advanced classes, counseling students, and research which included the following studies: inhibiting substances produced by plant pathogens; orchard diseases; soybean diseases and diseases of forage crops; root troubles of soybean and cereal crops, and the feeding of moldy corn to farm animals. Dr. Kent also worked with Dr. I. E. Melhus in the publication of a volume entitled "Elements of Plant Pathology."

Dr. Harold H. Williams, associate director of the Research Laboratory at Detroit of the Children's Fund of Michigan, has been appointed professor of biochemistry. Dr. Williams, before becoming associated with the Children's Fund, served as a research student in the Institute of Animal Nutrition at Pennsylvania State College and later in the laboratory of animal nutrition at Cornell University. Dr. Williams is co-author with Icie G. Macy of the book entitled "Hidden Hunger," published by the Jaques Cattell Press.



DR. HAROLD H. WILLIAMS
PROFESSOR OF BIOCHEMISTRY, CORNELL UNIVERSITY

IRA L. BALDWIN, NEW DEAN AT THE UNIVERSITY OF WISCONSIN

University of Wisconsin Photographic Laboratory
DR. IRA L. BALDWIN
DEAN AND DIRECTOR, COLLEGE OF AGRICULTURE, UNIVERSITY OF WISCONSIN

DR. IRA L. BALDWIN, the new dean of the College of Agriculture of the University of Wisconsin, is a native of Oxford, Indiana. He was graduated from the Purdue School of Agriculture in 1919 and immediately joined the faculty. He obtained his master's degree two years later, remaining at Purdue until 1925, when he went to the University of Wisconsin, obtaining his doctorate in 1926. In 1927 Dr. Baldwin resigned at Purdue to join the faculty of the University of Wisconsin, where he became professor of bacteriology.

He has served successively as chairman of the department of bacteriology and assistant dean of the School of Agriculture, and for the last year served as dean of the Graduate School, a post he relinquished on September 15, to become dean and director of agriculture.

Dr. Baldwin was a second lieutenant of field artillery in World War I and served as consultant to the Army and Navy from 1942 to 1945. He is the author of many scientific articles and co-author of a book in the field of bacteriology.

APPOINTMENT OF DR. PADY AT THE KANSAS STATE COLLEGE

DR. STUART M. PADY has joined the staff of the Department of Botany and Plant Pathology of the Kansas State College. For the past nine years he has been assistant head of the Department of Botany and Plant Pathology of Kansas State College.

From 1936 to 1943, Dr. Pady was head of the Department of Biology at Ottawa University, Kansas, where he was actively engaged in teaching and in research on mycology. From 1943–45 he worked as pathologist for the Emergency Plant Disease Prevention program under the Bureau of Plant Industry, stationed at Manhattan, Kansas, of the United States Department of Agriculture, and covering the two States Kansas and Nebraska. He took up the work in the Department of Botany and Plant Pathology on the Agricultural Experiment Station Staff of the Kansas State College with the rank of assistant professor of botany in September. The work at the present time consists of part-time teaching, mainly general botany, part-time research work, with some time given to mycological collecting and building up the mycological and pathological herbarium. It is hoped that extensive mycological and pathological collecting will be undertaken throughout the State in the near future. The research program is expected to be centered in certain pathological-mycological phases of cereal diseases. He expects later to give courses in mycology at the college.

A SCIENCE CABINET

BERNARD M. BARUCH, testifying recently before the Congress, urged the establishment of a "Science Cabinet," charged with the task of keeping America constantly prepared scientifically, this cabinet to consist of men selected solely on their eminence in science. He proposed the appointment by the President of an executive manager, to be nominated by the board with confirmation by the Senate. He recommended that full powers and responsibility should rest with the board, which would decide the program of research to be followed year after year. The chief objects to be attained are:

"1. Greatly increase our scientific brainpower, using scholarships and other aids to develop new scientific talent in American youth.

"2. Continued, vigilant military research in developing new weapons for national defense.

"3. Intensify science's war against disease.

"4. Offset the depletion of our natural resources.

"5. Stimulate basic scientific research to assure a continued flow of new, fundamental, scientific knowledge.

"6. The widest, most efficient spread of scientific information, both of as yet undisclosed war discoveries and future findings.

"7. Create a new, permanent, government agency to coordinate these many activities into an integrated, national, scientific policy.

"8. This agency to provide the expert counsel for the freest, world-wide exchange of scientific knowledge and for effective policing of the war-geared science of Germany and Japan."

THE TWENTY-FIFTH ANNIVERSARY OF THE PLANT INSTITUTE OF THE OHIO STATE UNIVERSITY

THE Plant Institute of the Ohio State University was organized within the College of Agriculture in 1921. Its membership includes the staff and graduate students of departments dealing with plants or plant products. The departments concerned include Agricultural Chemistry, Agronomy, Botany and Horticulture and Forestry. Other departments of the university regularly invited to participate in the meetings include Agricultural Economics, Agricultural Engineering, Bacteriology, Zoology and Entomology (including Genetics), and also the Ohio Cooperative Wildlife Research Unit.

The purpose of the institute has been to foster (1) an enlarged college program of research with plants; (2) freer cooperation among the departments concerned; and (3) closer coordination of effort among those working with plants.

During the twenty-five years of operation the Plant Institute has concerned itself with various projects. Of these the holding of semi-monthly meetings has been the enduring one—the project which has made possible a better understanding of the plant in all its aspects and roles, and greater inspiration and fellowship to the plant science workers themselves. Through this medium the facts, perspectives and problems of the plant science fields have been mutually shared. Other scientists who have come to the campus from time to time as guests of the institute and the graduate school, have enriched the annual programs and have added greatly to the vitality and service of the institute to its members and friends.

The program of 1945–1946 is being planned by the executive committee in celebration of the twenty-fifth anniversary of the institute.

EMERGENCY PLANT DISEASE PREVENTION

DURING his employment in the Emergency Plant Disease Prevention Project in Washington, D. C., from August of 1944 to July, 1945, the writer's chief duty was to serve as consulting diagnostician to twenty-four pathologists engaged in making plant surveys throughout the United States. Each of these twenty-four pathologists made individual surveys of the one or more States which were assigned to him and all forty-eight States were covered in this manner. The primary object was to determine what diseases of economic plants were prevalent in these areas and in what severity they occurred, and to suggest methods of control. If a disease could not be recognized by the field pathologist and if the causative organism could not be determined, some of the diseased material was sent to one of two main offices—one at Stillwater, Oklahoma, and the other at Washington, D. C.—where adequate laboratory and library facilities made possible the diagnosis of most of the diseases. The writer's duties included the identification of many such specimens, and this often required isolation and culture of the pathogenes, which could not be otherwise identified.

Many hundreds of specimens were received by the writer from the field pathologists. As soon as identifications were made, they were reported to the field pathologist, whose duty was then to prepare a report for publication in the widely distributed government periodical, the *Plant Disease Reporter*. The pathologist also kept in close contact with the State agents and made available to those agents whatever information he had obtained on the prevalence of the various diseases and their severity. This information made possible the devising of effective means for reducing crop losses by these diseases and means for preventing the spread of a disease from one area to another.

Another important function of the project was to keep check on any new diseases which may have come in by various means from some other country and obtained a foothold here. The writer examined a number of such diseases which had apparently not been heretofore reported in this country. In addition to this, a few new diseases, apparently not heretofore reported or described, were received and studied by the writer. Articles describing the disorders and the pathogenes responsible for them have been published or are awaiting publication.

LINDSAY S. OLIVE

DIVISION OF BIOLOGICAL SCIENCE,
UNIVERSITY OF GEORGIA,
ATHENS

THE LEDERLE GRADUATE RESEARCH FELLOWSHIPS

Two graduate research felowships have been established by the Lederle Laboratories at the Philadelphia College of Pharmacy and Science, of which Dr. Ivor Griffith is president.

These fellowships extend over a period of two years and afford the recipients the opportunity to do original research and, at the same time, complete the prescribed studies leading to the master's degree. Inclusive of tuition, the value of the awards is well over $1,000 per year.

The current recipients are Kenneth Avis, of Elmer, N. J., a graduate of the Philadelphia College of Pharmacy and Science, just returned from two and one-half years' service with the United States Navy; and Stanley Scheindlin, Philadelphia, Pa., a graduate of the School of Pharmacy of Temple University and winner of a number of awards from that institution.

NEUROLOGY AND PSYCHIATRY AT THE OHIO STATE UNIVERSITY

A DEPARTMENT of neurology and psychiatry will be established in the College of Medicine of the Ohio State University of which Dr. Dwight M. Palmer is chairman. The change goes into effect immediately.

Dr. Palmer, who has been given the rank of full professor, has been on the medical staff of the university since 1927. After two years as an assistant, he became an instructor in 1929, an assistant professor in 1934 and an associate professor in 1942.

A native of Scio, Harrison County, Ohio, Dr. Palmer was graduated from the high school there. He has three degrees from the Ohio State University, all with honors—the bachelor of science in 1920, the doctor of medicine in 1932 and the master of science in 1933.

He has specialized in psychiatry and neurology. During the war he lectured to physicians at many Army installations.

Other members of the faculty of medicine, some of them still on war leave, who are being assigned to the new department include: J. F. Bateman, George T. Harding, Frank F. Talman, Harry E. LeFever, professors; Harrison Evans, Franklin C. Wagenhals, associate professors; Roy J. Secrest, assistant professor; Paul C. Clark, Charles L. Anderson, Edward Humphreys, Attilio Laquardia, Dorothy E. Donley, Leonard M. Dub, Nicholas Michael, instructors; J. J. Alpers, Robert A. Kidd, Jr., John W. Wittenbrook, J. C. Price, assistants.

BIOLOGY AT THE UNIVERSITY OF ALABAMA

THE Department of Biology of the University of Alabama, of which Professor J. Henry Walker is head, the work of which was formerly housed in three buildings, will, after March 1, be carried out entirely in Nott Hall.

Sergeant C. H. Wolfe, general superintendent of the physical plant of the university, has been appointed supervisor in collaboration with Robert F. Baird, Sergeant J. B. Gregory, John D. Hendrix and Willard E. Ward.

The building, formerly occupied by the Medical School, is now being completely remodeled. The new quarters of the department will include three lecture rooms; twelve standard-unit laboratories with accompanying preparation rooms; ten offices, and special rooms for graduate studies, seminar, museum, herbarium, and rooms for specimens, for constant temperature, charts and lantern slides, chemicals and glassware.

Fluorescent lighting will be used throughout the building, which is being rewired and repainted. The ceiling will be covered with acoustical board, and semi-fireproof flooring will be added to supplement the sprinkler system with which the building is already equipped for fire-prevention. As an added precaution the wooden stairways are being replaced with steel and concrete stairs.

Special laboratories have been designed for parasitology, anatomy, physiology, cytology, histology and micrology. A two-story brick building in the rear of the building for biology will provide rooms for various types of research. Frame buildings to be used as animal houses are also provided.

The new home of the department meets the standards of some of the best in the South, and it will be the first time since 1927 that its work has been centralized in one building. Previously, classes were conducted in Clark and Smith Halls, and also in a small research laboratory in the rear of the Medical Building.

SHORT NOTES

An honorary degree was conferred at the eighty-first commencement of Purdue University on Dr. Ira L. Baldwin, dean of the College of Agriculture of the University of Wisconsin, in recognition of his "outstanding work in the fields of science and agriculture." (See page 34.)

At the fifth anniversary meeting of the Cleveland Health Museum presentation of the Elisabeth Severance Prentiss National Award in Health Education was made by radio over the National Broadcasting Company Network from New York by Dr. James Rowland Angell, president emeritus of Yale University, to Dr. Charles-Edward Amory Winslow, Ph.D. In accepting this award which was established by the museum to stimulate interest in health education, Dr. Winslow gave an address on "Ten Unattained Goals of Public Health." Dr. Winslow has taught public health at Yale Medical School for thirty years, was the first director of a Division of Public Health Education in the United States (New York State), and has served the Red Cross Societies, the League of Nations, and many national and international organizations in the interest of public health.

Dr. William B. Sarles, professor of agricultural bacteriology at the University of Wisconsin, has been appointed part-time assistant to the President of the University, Dr. Edwin Broun Fred. For the past thirty-two months on active duty in the United States Naval Reserve, he held the rank of Commander at the time of assignment to inactive duty on December 9, 1945. At the start of his period of active duty in March, 1943, Dr. Sarles was associate professor of agricultural bacteriology at the University of Wisconsin, and was secretary-treasurer of the Society of American Bacteriologists.

On October 12 Drs. Manuel Martínez Baez, undersecretary of public health and welfare, and Francisco de Paula Miranda, director of the National Institute of Nutrition, took one of the Pan American clippers for Quebec to attend, as members of the Mexican delegation, the second International Conference on Agriculture and Nutrition, to be held in Quebec.

Dr. R. H. K. Foster has become associate professor of pharmacology at the School of Medicine of St. Louis University. He was formerly, for ten years, chief of the Pharmacological Research Laboratories of the Scientific Department of Hoffmann-La-Roche, Inc., Nutley, New Jersey.

Dr. John S. Niederhauser has been appointed assistant professor of plant pathology at Cornell University to succeed Dr. M. F. Barrus, retired. He took up his new work on October 1.

Two new faculty members will take up teaching at the beginning of the winter quarter in the division of botany of the department of biology of Vanderbilt University in January. Dr. Harold C. Bold, formerly on the faculty of Barnard College, Columbia University, and more recently a lieutenant commander in the Navy, will become associate professor of botany. Dr. Fred T. Wolfe, who for the past three years has been on leave from his position as assistant professor of botany for duty in the Army, will take his place.

Grants from the Penrose Fund of the American Philosophical Society, Philadelphia, include $500 to R. Buetner and T. Cunliffe Barnes, Hahnemann Medical College, for a "General Explanation for the Therapeutic Action of the Drugs Which Act on the Nervous System"; $500 to Alfred F. Bliss, Albany Medical College, for a study of the Chemistry of Color Vision; $2,422 to Otto Meyerhoff, the University of Pennsylvania Medical School, for (a) Studies of the Mechanism of the Oxidation of Triosephosphate by Means of Cozymase, (b) Isolation of labile enzymes especially adenylpyrophosphatase from yeast. From the Johnson Fund a grant of $8,000 has been made to the Academy of

Natural Sciences of Philadelphia, in aid of the publication of the second and final volume of a study by Henry A. Pilsbry, entitled "Land Mollusca of North America."

Dr. John C. Krantz, Jr., professor of pharmacology of the School of Medicine of the University of Maryland, has received a grant of $2,000 from the Eaton Laboratories of Norwich, N. Y., in support of the general research program of the department.

A research grant of $2,000 for a study of the anti-pernicious anemia principle in liver from Armour and Company, Chicago, has been received by the Department of Biochemistry of the College of Medicine of the Universtiy of Vermont, of which Dr. H. B. Pierce is head. The research will be conducted by Dr. E. E. Hays, who was recently appointed assistant professor of biochemistry, of the School of Medicine of St. Louis University, where he worked in the department of biochemistry under Dr. E. A. Doisy. Dr. Hays has had experience in this field, having engaged in similar research at the University of Chicago.

Dr. R. E. Buchanan, director of the Agricultural Experiment Station at Iowa State College, has been named chairman of the technical advisory committee of the delegation of the Iowa Development Commission. He was a member of the delegation from the United States advisory to the Secretary of Agriculture, who was the official delegate to the Food and Agriculture Organization in Quebec.

Major Allan Hemingway, until his entrance into the AAF School of Aviation Medicine early in 1943, was assistant professor of physiological chemistry in the Medical School of the University of Minnesota, is now on the inactive list of the Army and will resume teaching and research. During the war Major Hemingway was in charge of the Biophysics Laboratory at the School of Aviation Medicine, Randolph Field, Texas. The general research program of the school consisted of research on selection and maintenance of aviation personnel. The Biophysics Laboratory functioned as a laboratory for consultation and cooperative work with other departments of the School of Aviation Medicine on all problems related to physics and engineering. Special problems of the Biophysics Laboratory consisted of physiological investigations related to the effects of motion such as motion sickness and the effects of exposure to high altitude, cold and anoxia.

Dr. Ralph S. Muckenfuss, director of the Bureau of Laboratories of the New York Department of Health, has returned to his work following a tour of duty as commanding officer of the First Medical General Laboratories of the European Theater of Operations. In recognition of his work in the Army he was awarded the Legion of Merit. The First Medical General Laboratory served as the central laboratory for the European Theater of Operations having much the same functions that the Army Medical School and Army Medical Museum have in the United States. The blood bank for sending whole blood to forward hospitals was organized in association with this laboratory.

The Public Health Research Institute of the City of New York, Inc., has announced that Dr. Ralph S. Muckenfuss who has recently returned from service with the Army, has been appointed director of the Institute and chief of the Division of Infectious Diseases.

Dr. Vincent W. Cochrane has been appointed assistant plant pathologist at the Connecticut Agricultural Experiment Station, to fill the vacancy left by the resignation of Dr. George A. Gries. Dr. Cochrane will study the root rot disease of ornamental and other plants, with particular reference to the microbiological population of the soil and the products of microbial activity as they affect plant growth. Until recently, Dr. Cochrane was a member of the staff of the Lederle Laboratories at Pearl River, N. Y.

The E. L. Patch Company, Boston, Mass., has announced the appointment of Dr. Charles A. Cook as research director at their laboratories in Stoneham. His previous experience has been concerned with the medical and biochemical properties of vitamins, proteins and amino acids, pituitary hormones and also synthetic and antibiotic chemicals that exhibit antiseptic activity. He graduated from the University of New Hampshire in 1927 and then obtained his Ph.D. from Yale, where he was assistant instructor in medical and graduate courses. Dr. Cook was formerly director of the department of medical biochemistry at The Lambert Pharmacal Company in St. Louis and prior to this was for over ten years in charge of the biochemical laboratory at the Burroughs Wellcome and Company research laboratories.

Dr. Paul Weatherwax, professor of botany at Indiana University, made an address on December 6 before the University of Cincinnati Chapter of the Sigma Xi on "Indian Corn and the Indian, Long Ago and Now."

An address by Professor Claude E. ZoBell, of the Scripps Institution of Oceanography, entitled "Microbial Transformation of Petroleum Hydrocarbons," was given at the three hundred and thirty-sixth meeting of the Washington Academy of Sciences, which was held jointly with the Geological Society of Washington on November 15.

Dr. Edwin J. Cohn, chief of the Blood Fractionation Laboratories at Harvard Medical School and chairman of the Department of Physical Chemistry at Harvard University, spoke on *Research in the Medical Sciences* on the evening of December 27, the fourth of the series of Laity Lectures of the New York Academy of Medicine devoted to the general subject of Medicine To-day. Dr. Cohn, well-known for his original and highly important work in blood fractionation, will present in his lecture a review of the progress which has been achieved in research in physics, in chemistry, in biology and in medicine. He will trace the relationship of these advances to our understanding of the organization of the body in health and of its disorganization in disease. Against this background he will undertake a review of the developments of research techniques which have taken place in the more recent years and their probable effects upon medicine in the near future.

In the lecture series "Recent Advances in Botany" now being given at the Brooklyn Botanic Garden, Dr. Ray F. Dawson, of the Department of Botany, Columbia University, was the first speaker, on December 12, 1945, on "Grafting as a Method of Study of Drug Production in Plants." Each lecture begins at 8:15 P.M. Forthcoming lectures in the series are: January 9. Control of Dormancy in Plants: Dr. F. E. Denny, Boyce Thompson Institute. February 6. Plants and the Material Basis of Civilization: Dr. E. W. Sinnott, Director of Sheffield Scientific School and Sterling Professor of Botany, Yale University. March 6. Effect of Light Periods on Flowering: Dr. C. B. Link, Horticulturist at the Brooklyn Botanic Garden. April 3. Plant Viruses and the Nature of Virus Diseases: Dr. B. M. Duggar, University of Wisconsin, and Lederle Laboratories.

A bequest from Miss Sarah Henderson of $1,075,000 for the endowment of the chair of tropical medicine and for the improvement and changes in the department, has been received by the School of Medicine of Tulane University, New Orleans.

Announcement has been made of ten one-year fellowships in psychiatry at the University of Michigan with an annual stipend of $2,000. These fellowships are under the aegis of the Office of Veterans' Affairs of the State of Michigan. The appointees will be trained at the Neuropsychiatric Institute of the university. Applicants must be graduates of a class A medical school and must complete a rotating interneship before becoming fellows.

BOOK REVIEWS[1]

EDITED BY
PROFESSOR CARL L. HUBBS

Embryologie Chimique. By JEAN BRACHET. Liege: Editions Desoer. Paris: Masson et Cie., 1944: 1–509, figs. 1–127. 420 fr.

STUDENTS of development, irrespective of special fields of study, should regard with enthusiasm the increased interest of systematists, geneticists, biochemists and physiologists in the events of individual development. All share a common objective with the experimental embryologist, *viz.*, the understanding of the events of ontogeny, and the reconstruction of the sequence of changes responsible for the production of an adult with characteristic structural and behavioral patterns. The difference is found in the immediate method or methods of attack. All make their direct contribution to our understanding of evolution, though the most obvious contributions in the past have been from the experiments of the geneticists.

Of the several books received from the freed countries of Europe which have been published since the liberation, one, "Embryologie Chimique," by Jean Brachet, of the University of Bruxelles, will be welcomed by all mentioned above. Dedicated to his father, who was a professor at the same university, the book is a summary and discussion of the physiology of development by one who, trained extensively in biochemistry, has devoted his time to embryology. It was planned and written during the German occupation and published immediately after the liberation of Belgium. In reading the book one must remember that Brachet was familiar with but a part of the work published outside of Germany and German-occupied countries after 1940.

While "Embryologie Chimique" may have some general scientific appeal it is, as Brachet states, addressed "*avant tout aux biologistes*" and adds, "*et nous souhaitons qu'il puisse attirer quelques-uns dans la voie fertile de l'embryologie chimique.*" The plan of the book, dealing as it does with the biochemistry and physiology of development, is almost unique in that he has chosen to hold "*au plan classique des livres d'embryologie: partant des cellules reproductrices, nous aboutissons à l'embryon doté de ses organes fondamentaux.*" The biochemical contributions are thus rendered more impressive and meaningful.

Following a section on the problems and the methods of chemical embryology, Brachet devotes a chapter each to sex-determination, gametogenesis, fertilization and segmentation. Each chapter ends with a short discussion of the conclusions which may be drawn from the work reviewed and a statement of the author's views. He then turns to a subject which has concerned him very directly, the synthesis, location and physiological rôle of the nucleic acids. This chapter he divides into two sections. The one deals with synthesis, in which, as in former publications, he discusses forms with partial synthesis and those with total synthesis. The other section, with a sub-section on the chromosomes, deals with the location of the acids and their functions, both in the resting cell and in the cell during mitosis. Here Brachet draws on material aside from the embryo and considers the relation between cytoplasm and nucleus. Chapter 7 contains a discussion of the energetics of development under the heading of growth, differentiation and metabolism.

Chapters VIII and IX, respectively, are devoted to the chemical embryology of the invertebrates and of one vertebrate group, the amphibians. As might be anticipated, the first deals primarily with the annelids and molluscs, the tunicates and the echino-

[1] Except as otherwise indicated, all items are prepared by the Review Editor, Dr. Carl L. Hubbs, Scripps Institution of Oceanography, University of California, La Jolla, California. All opinions are those of the reviewer.

derms. These forms, while well known to Brachet, experimentally are not as familiar as the Amphibia, on which more of his own experimental analyses have been based.

Since the publication of the classical paper by Spemann and Hilda Mangold in 1924, the investigations of many experimental embryologists have been concerned with the organization-center and induction. Chapter X, in which these subjects are presented, is the longest in the volume. After a preliminary statement, the cytochemical studies on the regions of the gastrula are summarized, the whole serving as a good introduction to the discussions of the metabolism of the "organizer" and the significance of its metabolism to induction. Two sections follow, the first dealing with the nature of the inducing substance and the second with the specificity of the inductors. In the first the author reviews quite impartially a field to which he himself has made many contributions. In the second he cautiously considers the question as to the nature of any specificity resident in the inducing material. He confines himself to a survey of the work of several investigators and a remark to the effect that the question remains "*bien obscure á l'heure présente.*" The chapter closes with the usual valuable discussion and conclusions.

After a brief chapter on the biochemistry of regenerating organisms, Brachet concludes with a review of those things which have been accomplished, those subjects which are controversial and those avenues of research which he believes may prove most fruitful. The chapter is suggestive.

"Embryologie Chimique" is a well-balanced, clearly written and well-documented book. Several times one regrets that a discussion is not more complete. From the standpoint of the objectives, that was obviously impossible. It can be hoped, however, that the book will lead some investigators into this fertile field and that, in addition, it may serve to inform some in neighboring fields of the accomplishments of the newer experimental approach to development. — LEIGH HOADLEY, *Harvard University, Cambridge, Mass.*

Physical Chemistry of Cells and Tissues. By RUDOLF HÖBER. With the Collaboration of David L. Hitchcock, J. B. Bateman, David R. Goddard, Wallace O. Fenn. Philadelphia and Toronto: The Blakiston Co., 1945: i–xiii, 1–676, figs. 1–70. $9.00.

FIVE outstanding authors, representing the fields of general physiology, biochemistry, physical chemistry, biophysics and plant physiology, have prepared in this substantial volume, a work which will be of wide interest and great value to research workers, teachers and advanced students in the newer school of physiology.

The subject-matter is "physiology as a branch of physical chemical science dealing with life as a physical, though exceedingly complex system, that may be subjected to scientific analysis like any other object." The successful purpose of the authors has been to emphasize, in logical sequence, the profound physicochemical bases through which various life processes are derived.

There are eight sections, most of which are each preceded by a short illuminating introduction. The subject-matter is clearly written, penetrating and critical. It is supported by adequate numbers of mathematical and chemical equations, structural formulae, tables, graphs, diagrams, simple drawings and a rich bibliography (there are 21 pages in the author index). The bibliography is presented in the form of footnote annotations, as in many other recent books. While this manner of presentation has advantages, one regrets the omission of titles of papers thus cited.

The first section (by Hitchcock) deals with certain important principles of physical chemistry, including diffusion in liquids, chemical reactions and their catalytic control, enzymic processes, outlines of thermodynamics, electromotive force, osmotic properties and colloidal phenomena.

Section 2 (by Bateman) considers the physicochemical, structural and functional

role of large molecules and molecular aggregates in living material.

In sections 3 to 5, Höber discusses the general architecture of protoplasm, the permeability of cell-membranes to water, acids and bases, polar and non-polar solutes including dyes, the physicochemical constitution of plasma membranes and the influence upon cellular activity of various outside factors.

Respiratory activities of cells and tissues are discussed by Goddard in section 6. Consideration of the nature, energetics and kinetics of respiration are followed by a treatment of the nature of oxidation, a substantial review of the various respiratory enzymes, and shorter chapters on the origin of carbon dioxide, relations between fermentation and respiration, and the utilization of liberated energy.

In section 7 Fenn discusses contractility, devoting a short chapter to a survey of contractile tissues in general and a lengthy one to the physicochemical structure of muscle and the chemical events in various phases of muscular activity.

Höber devotes the eighth and final section to a consideration of recent advances in studies of the passive penetration and active transfer of materials in tissues of animals and plants. Selected topics include intestinal absorption, kidney function and the elaboration of urine, general permeability and osmotic processes of the body surfaces of plants and animals, the secretion of digestive fluids, and the energetics and mechanisms of active transfer of solutions.

Notwithstanding the number of pages in the book the volume is of normal, handy size, as the paper is fairly thin. The print is clear and very readable. The binding is excellent and the covers stout and washable.— DENIS L. FOX, *Scripps Institution of Oceanography, University of California, La Jolla, Calif.*

Microbes of Merit. By OTTO RAHN. Lancaster, Pa.: The Jaques Cattell Press, 1945: i–viii, 1–277, frontisp., figs. 1–101. $4.00.—In a census of the bacterial population of the United States taken by a most ingenious method, the author arrives at the conclusion that out of every 30,000 bacteria 29,999 are harmless, useful or even necessary to our well-being. Compared with the human race, of which one out of every 17,000 in the United States was convicted of murder in 1942, bacteria present a pretty good record. The foregoing example illustrates the way in which Dr. Rahn has humanized microbes of merit in a style which is both entertaining and instructive. Discussed under this heading are the main groups of bacteria, yeasts and molds which are beneficial to man in various and sundry ways. Particular emphasis is placed upon the microbes which act as scavengers, aid animals in the digestion of food, contribute to soil fertility, function in the cycle of elements, and upon those which are of economic importance. Sometimes blundering bacteria cause the spoilage of food, but others contribute directly or indirectly to our daily bread. Not only do they contribute by aiding in the production of cheeses, butter, sauerkraut and pickles; their richness in proteins, fats, minerals and vitamins suggests the possibility that microbes may be the food of the future. The chapter on the role of yeasts in the production of wine, beer, alcohol, whiskey and other fermentation products by itself is sufficient justification for the book. Although "bacteria have no brains" and a few have bad reputations, in an epilogue the author pictures life on an imaginary earth without microbes as hardly worth living.

This popularized account of beneficial bacteria will be of interest to the lay as well as to the professional microbiologist. Excellent analogies and new viewpoints are intriguing. The text is profusely illustrated with carefully selected photographs, microphotographs, drawings and subtle cartoons. Reading the book will prove to be both pleasant and profitable. Although the author has characterized the appearance of bacteria as uninterestingly monotonous, he has done a good job of vitalizing the activi-

ties of microbes of merit.—CLAUDE E. ZOBELL, *Scripps Institution of Oceanography, University of California, La Jolla, Calif.*

Plant Growth. By L. EDWIN YOCUM. Lancaster, Pennsylvania: The Jaques Cattell Press, 1945: 1–203, figs. 1–25, pls. 1–16. $3.00.—The difficult task of presenting the facts of plant growth in palatable form to the intelligent layman and plant lover has never been very successfully solved by plant physiologists. Professor Yocum has endeavoured to remedy this condition, and his book must be judged by how well it does the job. In the words of the author, "It is an attempt to make clear the 'how and why' of plant growth."

The book would probably provide the enthusiastic Victory gardener with the answers to many of his problems if he possessed the patience and tenacity to wade through twenty-five assorted chapters with titles ranging from "Cell Structure and Protoplasm" to "Insects and Diseases." However, it is more than likely that he would have become swamped in a mass of detail without having achieved his objective, or having obtained a clear picture of the principles of plant growth.

It is questionable whether the conventional linkage map of the chromosomes of *Drosophila* has a useful place in a book of this nature. A more fortunate arrangement of textual material with the plates would have enabled the reader to turn directly from plate to text; e.g., Plates VIII and IX are inserted between pages 118–119, but are referred to on pages 147–148. There is reason to doubt that some of the plates (Plate VI and Plate XVI) illustrate very clearly what the author had in mind. References for advanced reading are given at the end of most chapters. There is a glossary and an index.—THOMAS W. WHITAKER, *U. S. Department of Agriculture, Box 150, La Jolla, Calif.*

Reptiles of the Pacific World. By ARTHUR LOVERIDGE. New York: The Macmillan Co., 1945: i–xii, 1–259, pls. 1–7. $3.00. Washington: The Infantry Journal, 1945: i–xii, 1–243, pls. 1–7. $0.25 (Service edition, paper).—A layman's field book on any group of animals is no better than the means of specific and generic identification that it offers, because few people enjoy reading about unfamiliar animals. By including only technical keys to the Pacific reptiles and amphibians Loveridge has missed an opportunity to help many a lonesome service man pass weary hours of isolation. The few illustrations are uninteresting and there is no map in the service edition, the inexperienced user being helped only by a distribution table. On the other hand, the text, made up largely of descriptions of habits, is accurate and interesting. Herpetologists and zoologists in the Pacific and elsewhere will find this work extremely useful.

One of the fourteen chapters deals with snake-bite and its treatment. Here the author has failed to consider the work of Australian investigators on the effect of elapid venom; what information he does give is not always up-to-date. For instance, he repeatedly advises the use of potassium permanganate although recent experiments indicate that its use may be more harmful than beneficial. He fails to warn against the indiscriminate use of antivenin made from horse serum. However, for these faults and omissions Loveridge should not be too severely blamed, since a more scattered, confused, and contradictory literature than that on snake-bite and its treatment is impossible to find.

The service edition has a paper cover, and is conveniently printed on light paper in pocket size.—CLIFFORD H. POPE, *Chicago Natural History Museum, Chicago 5, Illinois.*

SECTION ON GENETICS AND EVOLUTION

CERTAIN ASPECTS OF REPRODUCTION IN A HERD OF SILVER FOXES

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FOR a number of years careful records have been kept of many phases of reproduction in the herd of silver foxes, *Vulpes fulva,* at the U. S. Fur Animal Experiment Station, Saratoga Springs, New York. Some of these records have been analyzed in this report in an attempt to present a quantitative picture of the process of reproduction in a species that has not long been subjected to domestication. The season of mating and the number of pregnancies each year have been affected in most of the laboratory and domestic animals by centuries of close association with man, but the silver fox has only been kept in captivity for about 60 years. The latest captive generation is nearly as intractable as the first.

The number of foxes treated in this report falls short of the total desired for high statistical reliability, but since we know of no comparable collection of data, we feel justified in publishing these extensive observations and in drawing certain conclusions from them. It must be kept in mind that foxes are seldom kept under exactly the same conditions year after year, least of all at an experimental station such as the one at Saratoga. The diet, method of mating and other management practices have undergone considerable experimentation since 1930, when observations on this herd were first recorded. The effects of such changes on some aspects of reproduction are hopelessly complex. We shall discuss here only those aspects which we feel have not been seriously affected by these variations in handling, or, when some experimental procedure obviously endangers the accuracy of the con-

clusions, we shall omit the experimental animals from the calculations.

The Time of Mating

Silver foxes have a single estrus lasting one to six days of each year. Usually it occurs during February, but in most years a few vixens will mate in January and a few others not until March. Since 1932 the earliest mating at Saratoga has been on January 10th and the latest on April 1st. Johansson (1938) reported an almost identical breeding season for foxes on several farms in Sweden. On one English farm matings extended from January 12th to March 19th (Rowlands and Parkes, 1936).

Most fox pups are whelped in March or April and males and females are capable of breeding in the next spring when they are about 10 months old. Fox ranchers have long known, however, that the yearling vixens tend to breed later than the adults, so in this study of the time of mating the yearling and adult averages have been kept separately. In each year the date of the first recorded mating of each vixen has been taken as the time of estrus, although in many cases she may actually have become receptive 2 or 3 days earlier. As an indicator of the beginning of estrus, therefore, the time of the first mating may be inaccurate by as much as 3 days in a few individual cases. The majority of the vixens in this study were mated on the first or second day of heat, however, so in the large groups considered here it may be assumed that the average time of mating is between the first and second day of estrus.

The mean mating date of all the adults during the last 13 years was February 12th. This average was obtained by finding the mean mating date of the adults in each year, then calculating the mean of these thirteen values. In this way each year was given equal weight regardless of how many or how few foxes mated in that year. If the division into years is discarded and all the mating

dates are added together, the mean is found to be February 13th with a standard deviation of 9.9 days. The mean mating date of the yearlings, calculated by the method of weighting each year equally, was February 21st, or 9 days later than the adult average. Discarding the division into years, the mean yearling mating date was February 20th with a standard deviation of 12.0 days (Table 1). Johansson (1940) stated that yearling females mated about 2 weeks later than adults. In a single year in a herd studied by Kellogg (1941), 256 yearlings whelped on an average 3 weeks later than 310 adults that were whelping their second litters. Thirty-four 2-year-olds bearing their first litters had an average whelping date intermediate between yearlings and adults with their second litters. Since gestation seldom varies more than one day from 52 days, the mean mating dates must also have differed by three weeks.

The standard deviations given above show that the time of mating among yearlings is more variable than among adults.

The problem of why yearlings mate later than adults is more complex than it appears to be at first glance. To say that the young simply do not mature quite soon enough to mate at the same time as the adults is to avoid the real problems that are involved. What organs are responsible for this immaturity, and why should they require longer to mature in some years than in others? Not only is the mean mating date of the yearlings quite variable from year to year, but it does not even bear a constant relationship to the mean adult mating date. It is always later than the adult mating date, but it may be seen in Table 1 that in 1944 the yearlings mated on the average 16 days later than the adults, whereas in 1941 they mated only 2 days later.

The difference between the mean yearling mating dates in different years might be accounted for in some years by a difference in the mean date on which the breeding yearlings were whelped. If all the yearlings that were

kept for breeding stock one year happened to be ones that were whelped late, it might be expected that they would, on the average, breed late. Johansson (1938) stated that there was no such tendency in the foxes studied by him, but among 194 yearlings in the Saratoga herd there was a low correlation between the date that a vixen was born and the date that she mated in her first

TABLE 1

MEAN MATING DATES OF THE SARATOGA HERD OF FOXES

Year	Adult vixens		Yearling vixens	
	Number averaged	Mean date of mating	Number averaged	Mean date of mating
1932	9	February 10	4	February 17
1933	11	" 3	10	" 12
1934	11	" 4	4	" 10
1935	10	" 13	10	" 19
1936	14	" 11	7	" 26
1937	19	" 18	15	" 22
1938	25	" 13	18	" 25
1939	39	" 22	20	" 27
1940	35	" 19	29	March 4
1941	48	" 17	32	February 19
1942	67	" 9	34	" 14
1943	57	" 11	20	" 19
1944	44	" 11	17	" 27

season ($+.27 \pm .07$)[1], which means that there was a slight tendency for early-whelped yearlings to mate early, and for late-whelped yearlings to mate late. This tendency was less distinct in the second year, and absent in the third. This confirms the statement made by Kellogg (1941) that a somewhat higher percentage of early-born yearlings whelp earlier first litters than do late-born yearlings. In spite of this tendency the mean mating date of yearlings was not always late in seasons after the mean birth-date of these same yearlings was late, nor early in seasons after early whelpings. The variation in the mean mating date of the yearlings from year to year, therefore, can not be explained by the slightly different whelping dates in different years.

The mean mating dates of 73 vixens that mated in each of their first 3 years were compared. Since these records

[1] The standard error will be used throughout this paper.

were spread over a 14-year period, each mating date was adjusted to compensate for the fact that the entire herd mated much earlier in some seasons than in others. All the adult mating dates in each season were advanced or retarded the necessary number of days to cause the mean for that year to fall on February 12th, the mean for all years, and yearling mating dates were adjusted to a mean of February 21st. This was merely a method of adjusting all years to the same level so that mating dates in different years would be more nearly comparable. These 73 vixens bred on the average 7 days earlier in their second year than in their first, and 2½ days earlier in their third than in their second year. This difference of 2½ days is only 1.9 times its standard error, and is not certainly significant, although additional data might prove it to be.

If the mating dates are not shifted to compensate for yearly variation, the results are little different. By this method of computation these vixens mated 6½ days earlier when they were 2 years old than when they were yearlings, and 3½ days earlier when they were 3 years old than when they were two.

There is apparently a slight tendency for sisters to mate at approximately the same time. The correlation between the mating dates of 80 pairs of adult sisters (littermates) after adjustment as above for differences in the mean mating date of the herd in different seasons, was $+.28 \pm .10$. Part of this correlation may depend upon the fact that the sisters necessarily were born on the same date. It was shown above that there was a definite correlation between date of birth and date of mating in the first year, but this does not apply to the present situation in which only adults were considered. The correlation between the date on which a vixen was born and the date on which she mated in her first adult season (her second breeding season) was $+.14 \pm .09$. There was no detectable correlation in the third year.

Factors Influencing the Time of Mating

Many experiments conducted during the past 15 years have led to the widespread belief that the increasing length of day in the spring is the stimulus that brings spring-mating animals into breeding condition. Undoubtedly this is a major factor. In the fox, however, it is probably not the only one because the mating season is considerably earlier some years than others, whereas the lengthening of the days is almost the same every spring. There is no doubt that the mean mating date of the adult herd varies significantly from year to year, even when the herd is composed of the same individuals in those years. Rather than assume that such a herd responds differently to the same light stimulus in different years, it seems safer to assume that some factor, or factors, other than light also influence the time of mating.

Temperature was investigated as a possible factor. The mean mating dates of the adults and yearlings in the herd for each year from 1933 through 1944 were compared with the mean, mean maximum and mean minimum temperatures of the preceding autumn and winter months. The months from September through February were compared both singly and collectively. No close relationship, however, between temperature and mating date was uncovered.

Of interest in connection with the effect of temperature and light on the time of mating is the observation that fox breeding in Scotland is not significantly later than in England (Rowlands and Parkes, 1936). Johansson (1940), however, stated that estrus appeared earlier in the south of Sweden than in the north. Obviously, more observations are needed on this important aspect of reproduction.

It is possible that different methods of feeding and managment have some effect on the time of mating. Some fox ranchers believe that allowing the males and females to be together during the proestrous period causes the vixens to come into heat sooner than if the

sexes had been kept separated. The males and females at Saratoga have been penned together for a few hours at frequent intervals during proestrus in every year except 1939. In that year the males were placed with the vixens only when a mating was expected, and as a result, the vixens were not subjected to the lively courting activity that occurs when the two sexes are together. The mean mating date of the herd was later in 1939 than in any other of the 13 years studied.

Although final proof is lacking that the presence of a male fox advances estrus, this association of the sexes may be the reason why foxes mate earlier in the wild than in captivity (Seton, 1909).

Individuality in the Time of Mating

A tendency for a particular vixen to mate at the same time each year is shown by the correlation between the mating date of each female in her first (yearling) breeding season and the average date on which she mated in the following years. The coefficient of correlation for 127 vixens was $+ .45 \pm .07$. This moderate correlation demonstrates a definite tendency for adult vixens to mate at the same time in the breeding season, relative to the other adults, as they did when they were yearlings, relative to the other yearlings. This fact is interesting because it shows that although yearling foxes mate later than adults, the mechanism that determines whether in later years they will mate early or late is already operating.

An analysis of the variance of the mating dates of adult vixens revealed a distinct tendency for individuals to mate at about the same date in each season. Table 2 summarizes the calculations and shows that, in general, individual vixens adhered more nearly to their own characteristic mating date than they did to the mean mating date of the herd.

A similar study of mink (Apelgren, 1941) revealed a tendency for females to have their young on the same

date year after year; the correlation was + .37. Since the length of gestation in the fox is very constant, the tendency for a vixen to whelp on the same date year after year can be measured by a correlation coefficient of approximately + .45—the same correlation as for the mating date, given above. It is apparent, therefore, that in both mink and foxes the date of whelping is, to a certain extent, a matter of individual expression.

TABLE 2

ANALYSIS OF VARIANCE OF MATING DATES OF INDIVIDUAL ADULT VIXENS IN TWO OR MORE YEARS

	Sum of squares of deviations	Degrees of freedom	Mean square deviation
Within vixens	11,975	218	54.93
Between vixens	17,360	90	192.9
Snedecor's F = 3.5		1 per cent. point = 1.5	

DURATION OF COPULATION

Copulation in the fox and dog are similar in that the mating pairs become coupled or "locked" together so that the penis can not be withdrawn voluntarily. After intromission has been accomplished the animals stand facing in opposite directions, unable to separate, although making frequent attempts to do so. They are able to separate, however, if they become excited at the approach of an observer, or if they are disturbed by unusual noises; but barring such an intrusion the attachment is secure enough to withstand considerable tugging. The "locking" together is said to be caused by contraction of the muscular bands of the vagina, together with a considerable enlargement of the glans penis after intromission (Johansson, 1940).

A record has been kept of the duration of 524 copulations in the Saratoga herd since 1939. The mean was 26.49 ± .46, with a standard deviation of 10.58 minutes. The shortest was less than 1 minute and the longest was 67 minutes. The large standard deviation indicates that there was a very wide spread in the duration of copulations: one third of the matings fell outside the 16 to 37-

minute period delimited by the standard deviation. The duration of the numerous matings of an individual were also quite variable.

It may be seen from Table 3 that males tended to remain coupled longer when they were adults than when they were yearlings. A similar trend is suggested by the data on females, but is not as pronounced.

TABLE 3

MEAN DURATION OF COPULATION OF FOXES. THE DIFFERENCES BETWEEN PAIRS OF MEANS MARKED * ARE TWO OR MORE TIMES THEIR STANDARD ERRORS

	Males			Females		
	Number of foxes	Number of matings	Mean duration in minutes	Number of foxes	Number of matings	Mean duration in minutes
All matings	43	524	26.49 ± .46	174	524	26.49 ± .46
All yearlings	29	132	22.59* ± .84	131	186	25.74 ± .82
All adults	35	392	27.83* ± .55	120	338	26.94 ± .56
Foxes that mated in their first 2 seasons:						
Yearling season.	18	112	23.36* ± .89	64	113	24.41* ± 1.02
Second season ..	18	165	28.44* ± .84	64	115	28.37* ± .94
Foxes that mated in their first 3 seasons:						
Yearling season.	9	41	23.34* ± 1.51	32	42	24.21 ± 1.36
Second season ..	9	82	28.03* ± 1.15	32	57	27.63 ± 1.19
Third season ..	9	61	30.72 ± 1.22	32	54	29.96 ± 1.45

To determine whether the lengthening of copulation occurred in the second or in later years, the adult years were subdivided and compared separately with the yearling year. All foxes that mated in their first two seasons were segregated, and the mean duration of coupling was calculated for the males and for the females in each year. The length of coupling was significantly longer in both sexes in the first adult year than in the yearling year. Similarly, all the foxes that mated during their first three years were picked out to form another group, and the mean duration of coupling in this group was longer in the third season than in the second for both males and females. The differences between these second and third-year means can not be said to be statistically sig-

nificant, but since in both sexes the third-year mean differed from the second in the same expected direction, it is not unlikely that additional data would prove that third-year matings were appreciably longer than second (Table 3).

It is not clear why adult matings are longer than those of yearlings. There is some indication that young animals are more restless and irritable while locked, thus tending to break apart sooner than adults; the latter seem to accept the situation more calmly. This does not lend itself readily to proof, however, and it is quite possible that some factor more closely related to the little-understood physiology of the locking mechanism is responsible. The figures presented in Table 3 are important for understanding this mechanism because they indicate that both the male and female are responsible for terminating the mating. If one sex played a totally passive role in the mating, the duration of copulation would not be expected to vary with age in that sex.

These same data are arranged in a different form in Table 4, where it appears that the male is much more

TABLE 4

MEAN DURATION OF COPULATION

	Duration in minutes	Number of matings
Adult males with adult females	27.57 ± .59	290
Adult males with yearling females	28.12 ± 1.06	109
Yearling males with adult females	22.53 ± 1.09	58
Yearling males with yearling females	22.33 ± 1.15	76

responsible for the duration of mating than the female, because the average duration of copulation of adult males was nearly the same whether they mated with adult or with yearling vixens. Similarly, copulations of yearling males averaged the same length when they mated with adult females, and when they mated with yearling females.

EFFECT OF DURATION OF COPULATION ON LITTER SIZE

Iwanow (1917) and Evans (1933) state that sperm are released slowly during the long copulation in the dog,

and the same is true for the fox (Starkov, unpublished manuscript, and Johansson, 1940). This leads to the speculation that one of the functions of the locking period might be to prevent parting of the couples before an adequate amount of semen had been deposited. Furthermore, the hydraulic action of the penis in the vagina while the pairs are locked might help the sperm to ascend the uterus and oviducts. These speculations about the importance of locking to fertility suggested that the size of the litter produced might depend in part upon the duration of the fertile copulation. Accordingly, the correlation between the duration of copulation and the size of the litters whelped by 109 vixens mated only once was calculated. The correlation coefficient was $-.01$, so there was obviously no relationship, and the speculations given above can not explain, therefore, the existence of the strange habit of locking during copulation.

There was also no correlation between the duration of copulation and the number of pups per mated female. This group differed from the group considered above by including females that mated but did not produce any young, or that destroyed their litters.

Individuality in Duration of Copulation

An effort was made to determine whether duration of copulation was a matter of individuality; that is, whether some foxes were consistently short-maters and others long-maters. An analysis of the variance of copulations of adult males with adult vixens is given in Table 5, and reveals a tendency for the copulations of individual males

TABLE 5

Analysis of Variance of Duration of Copulations of Adult Foxes

	Sum of squares of deviations	Degrees of freedom	Mean square deviation
Within males	16,983	206	82.44
Between males	7,736	24	322.33
Snedecor's F = 3.9		1 per cent. point = 1.9	
Within vixens	15,853	164	96.66
Between vixens	8,931	66	135.32
Snedecor's F = 1.4		5 per cent. point = 1.4	

to be of characteristic length. A tendency for adult vixens to express individuality as to duration of copulation when mating with adult males is also suggested, but can not be considered firmly established.

Size of the Vulva

The vulva of the fox increases in size and tumescence as estrus approaches, and is a fairly reliable indicator of the sexual condition of the vixen (Bassett and Leekley, 1942). The greatest size is reached and regression begins one or two days before the female becomes receptive.

The vulvar size was estimated with the help of a numbered set of ten oval wire loops of graduated sizes. In the beginning the examiner recorded the number of the loop that would barely enclose the vulva, but after experience had been gained in "freehand" estimation the loops were used infrequently. Size 3 ($7/16 \times 10/16$ inches) was the smallest encountered during the breeding season, and size 9 ($14/16 \times 18/16$) the largest.

A study of the vulvar size of the fox has already been published (Pearson and Bassett, 1944), where it was shown that vulvar size was related to the number of pups per mated female, to the per cent. of mated vixens failing to whelp, to the survival of litters and probably to litter size. Superior performance was associated in each case with large vulvar size.

We have made an analysis of the variance of vulvar size among foxes, making use of the new data that have become available since the publication of the above papers. The calculations are summarized in Table 6,

TABLE 6

Analysis of Variance of Vulvar Size of Adult Vixens, 1940–1944

	Sum of squares of deviations	Degrees of freedom	Mean square deviations
Within vixens	89	115	.774
Between vixens	107	62	1.726
Snedecor's F = 2.23		1 per cent. point = 1.65	

where it is shown that there was a strong tendency for the vulva of an individual adult vixen to reach the same

maximum size in successive seasons. It was shown in the earlier paper that there was a definite correlation also between the size of vulva in the yearling year, and the size in subsequent years, in spite of the fact that the vulva is smaller in yearlings than in adults. This is of interest because it shows that whereas a yearling vixen has not matured to the extent that she develops an adult-sized vulva, her individuality has developed to such an extent that one can forecast with some reliability the characteristic size that her vulva will attain in subsequent years.

Destruction of Litters

One of the most puzzling problems confronting fox ranchers is the tendency for many vixens to kill their young during the first few days after whelping. The young may be entirely eaten by the mother, or carcasses and dismembered pieces may be found lying about the pen. Numerous attempts have been made to determine the cause of this behavior, which frequently reaches serious proportions. In data collected at the Saratoga Station, no distinct correlation has been found between the destruction of litters and a number of other factors such as diet, weather at the time of whelping, temperament of vixens and earliness or lateness of whelping. Unusual disturbances at the time of whelping probably induce some vixens to destroy their litters, and abnormality of the young may also result in their destruction in some cases.

Table 7 shows that the tendency to destroy litters does not diminish as the vixens become older. For example, the rate is about the same in all four years in the group that was kept that long. The improvement of the 4-year group over the 3-year group, and the 3-year group over the 2-year group results from killing many of the litter-destroyers after their second or third breeding season. That such selection by the ranch manager can be effective is shown by studying the second-year records of vixens that destroyed their litters as yearlings, and comparing

them with the other two-year-olds in the herd. There were twenty-three such vixens that destroyed their litters in their first year and that were kept over until the next breeding season. Eighteen of these whelped litters the second year also, but 61 per cent. of these eighteen again dstroyed them. This figure should be compared with the average litter destruction by all second-year vixens bearing litters, which was 29 per cent. There seems little doubt, therefore, that some vixens have a strong and lasting tendency to destroy their litters.

TABLE 7

PER CENT. OF FOX LITTERS DESTROYED BY THE MOTHER

Group	Year	Number of litters whelped	Per cent. destroyed
Vixens kept 2 years or more	1st	93	26
	2nd	118	29
Vixens kept 3 years or more	1st	36	17
	2nd	50	10
	3rd	46	13
Vixens kept 4 years or more	1st	31	6
	2nd	36	0
	3rd	32	3
	4th	32	9

The breeding record in the second year of these vixens that destroyed their litters as yearlings is entirely normal except for the high per cent. of litters destroyed. But this undesirable tendency is so strong that it is obvious that these individuals should be pelted before the next breeding season.

PRODUCTIVITY

Productivity will be considered under three headings: the number of pups per litter, the number of pups per mated female, and failure to mate.

(1) *The number of pups per litter*

The mean number of pups per litter whelped by 100 yearling vixens from 1930 through 1944, omitting all destroyed litters, was $4.16 \pm .15$. There were from one to eight pups in each litter, and the standard deviation was 1.52 pups. The mean number of pups in 254 litters

whelped by adult vixens during the same period was $4.59 \pm .10$. Litter size ranged from one to nine, with a standard deviation of 1.56 pups. The pups were counted when about 7 to 10 days old; pups disappearing before this time would not, of course, be included in the count. The difference between adults and yearlings of .43 pups per litter is 2.4 times its standard error, which is sufficiently significant to warrant a more detailed study of the probable causes of this difference.

The larger size of the litters whelped by adults might result from two causes: A, an actual increase in the number of young born or raised by the same mothers in successive seasons, or B, an apparent increase in litter size due to selection. Vixens that produce large litters are generally kept for breeding the next year, but poor producers are pelted.

The first cause, item A above, is easily approached by examining the breeding records of a group composed of the same vixens over a period of years. This procedure eliminates the element of selection within the group because every animal is kept over the entire period. Sixty vixens whelped litters in their yearling and second year; the mean number of pups was 4.4 for both ages. Similarly, in determining whether there was a change in the third year, 35 individuals were included that whelped in their first 3 years. The mean litter sizes were 4.4, 4.6 and 4.4. Again there was no increase of litter size with age. Twenty vixens that whelped in each of their first 4 years were studied, and the mean sizes of the litters were 4.5, 4.6, 4.6 and 4.9. Sixty-two litters whelped by vixens 5 to 10 years old averaged 4.9 pups. The apparent increase in the fourth year is not statistically significant, although more data might prove it to be so. Actually, it is hard to picture a situation in which there is no increase in litter size during the first 3 years, then an appreciable increase in the fourth. A small increase each year would seem more likely, but can not be demonstrated with the data available.

Let us examine part B mentioned above: the effect of selection on the mean litter size of the herd. At Saratoga, selection of the animals to be kept for breeding stock has been based chiefly on the value of the pelt and the productivity of the individual. So many vixens had to be pelted, however, because they had poor pelts or failed to become pregnant, that selection for large litters could not be carried out rigorously without depleting the herd. Consequently, a vixen that whelped three pups had nearly as good a chance of being kept for breeding as one that whelped seven. For this reason there are no figures showing how effective selection actually is. However, some evidence is given below that selection for large litters can not be expected to be very effective.

The rate at which selection for increased litter size can raise the herd average is limited by the extent to which each vixen tends to have the same number of pups each year. Obviously, if there is no correlation between the size of the litters whelped by a vixen in different years, then selection has no foundation upon which to work and is useless. An analysis of variance of litter size among 61 adult vixens revealed a slight but significant tendency for individual adults to bear litters of the same size in several seasons (Table 8). However, the coefficient of

TABLE 8

ANALYSIS OF VARIANCE OF THE SIZE OF LITTERS WHELPED BY ADULT VIXENS

	Sum of squares of deviations	Degrees of freedom	Mean square deviation
Within vixens	209	152	1.37
Between vixens	252	60	4.20
Snedecor's F = 3.05	1 per cent. point = 1.6		

correlation between the size of the litter whelped in the yearling year and the average litter size in subsequent years for 67 vixens was only $+ .20 \pm .12$. This is a low correlation with doubtful statistical significance, showing that there is very little, if any, tendency for individual vixens to have the same number of pups in their adult years as they did in their first. Consequently, it would

be difficult to increase the average litter size of the herd by selection of these females, even if litter size were hereditary—an inheritance still to be proved. An analysis of variance revealed no significant tendency for adult sisters (littermates) to whelp litters of the same size.

In an analysis of variance of a larger sample of silver foxes, Johansson (1938) stated that there was a slight tendency for a certain litter size to be characteristic of an individual vixen. He concluded, however: "Most of the variation in litter size in the fox is, apparently, due to environmental influences. A selection for larger litter size would probably yield some results, but it would be a rather slow process, and the increase could not be expected to be very large." Apelgren (1941) concluded that among mink females, the size of the litter depended at most 10 per cent. on the individual qualities of the female, and 90 per cent. on environmental influences. The correlation between size of first and later litters among guinea pigs was only + .10 (Eaton, 1932).

There was no tendency for the several litters sired by an individual male mated with adult vixens to be of the same size (Table 9).

TABLE 9

ANALYSIS OF VARIANCE OF THE SIZE OF LITTERS SIRED BY ADULT MALES MATED TO ADULT FEMALES

	Sum of squares of deviations	Degrees of freedom	Mean square deviation
Within males	240	88	2.73
Between males	53	17	3.12
Snedecor's F = 1.14		5 per cent. point = 1.8	

(2) *Number of pups per mated female*

The number of pups per mated female is always considerably less than the number of pups per litter because many vixens fail to become pregnant, resorb their embryos, abort, or kill their pups shortly after birth. In the following figures, vixens that destroyed their litters have been considered the same as non-producers. From 1930 through 1943 the mean number of pups produced by 193 mated yearling vixens was 1.89, while the mean

number of pups resulting from all 312 adult matings during this time was 3.16. Obviously, the adults were more prolific than the yearlings, but we must discover whether the increase was due to better production by adult foxes, or to elimination of many non-producers before they became adults.

To solve this problem the records of 67 vixens that were kept for at least 3 years were picked out. They averaged 3.2 pups in their first year, and 3.5 and 3.4 in the next two. The slight improvement with age is of doubtful significance in view of the large difference of 1.27 pups for which we are trying to account. In a group composed of the 37 vixens that were kept for at least 4 years, the mean number of pups in each of the years was 3.8, 4.1, 4.0, and 4.2. Again the evidence for improved productivity with increasing age is slight. Obviously, the improvement noted above must have been due to selection.

It has been shown in the previous section that there was very little emphasis placed upon increasing the average litter size of the group through selection of vixens that whelped the largest litters—a process of doubtful effectiveness. Therefore, the increased productivity that was obtained by selection must have been due to the elimination of vixens that habitually destroyed their litters or that failed to whelp. It was demonstrated above that many vixens tended to destroy their litters year after year, so it is apparent that this is one point where selection can work effectively to increase production. It remains only to be demonstrated whether production can be increased by pelting vixens that mate but fail to whelp.

The category "failed to whelp" includes three subdivisions which, with the data available, could not be separated satisfactorily. These are failure to become pregnant, resorption of embryos and abortion. Vixens would also have been recorded "failed to whelp" if they had eaten their entire litter without leaving any blood or other trace. Between 1936 and 1943, 37 per cent. of 179

mated yearlings failed to whelp for one of these reasons. Seven per cent. were known to have aborted, and there may have been other abortions that were not detected. During the same period only 15 per cent. of mated vixens in their second year and 16 per cent. of all mated adults considered together failed to whelp. Of 31 mated vixens that failed to whelp in their first year, but that were kept over and mated the next, 23 per cent. failed to whelp again. This is more than the expected 15 per cent., but not nearly as great as the 37 per cent. among yearlings. In other words, failure to whelp showed some tendency to recur in the same vixens, but this tendency was not nearly as strong, for example, as the tendency to destroy litters. Nevertheless, it is usually advisable to pelt yearlings that fail to whelp, not only because their production is often subnormal the next season, but because failure to whelp in the first year may be a hereditary condition that can be passed on to the offspring. If this is so, and no attempt has been made to prove it, allowing unproductive yearlings to mate in their second year might perpetuate a strain of foxes that was always unproductive in the first year.

(3) *Failure to mate*

Failure to mate is sometimes not the fault of the vixen, but may be due to inactive, non-aggressive males. Often the rancher may be at fault for failing to introduce a male at the proper time. A man, by examining the vulva and watching the behavior of the vixen, can usually tell when she is receptive, but he is by no means as skilful at detecting receptivity as an aggressive male fox. In some cases, therefore, estrus passes undetected, but in others, the vixen fails to come into heat at all, or the symptoms are so slight that they fail to arouse the interest of even an aggressive male. Since it is frequently impossible to tell which of the above factors is responsible for failure to mate, no attempt will be made to separate them.

From 1936 through 1943, 16 per cent. of 213 yearling vixens failed to mate. During the same period 6 per

cent. of the 2-year-old vixens failed to mate, and only 2 per cent. of those older than 2 years. The corresponding percentages on several Swedish farms were 15, 4 and 6 (Johansson, 1938). Obviously there was a marked improvement with the passage of time, but again it must be determined whether the improvement was due to maturing of the individuals or to pelting many of the vixens that failed to mate. If, as before, we select all the vixens that were kept for two years or more, and compare their records for the first two years, we find that in the first year 14 per cent. of the 132 vixens did not mate, while 7 per cent. failed to mate in the second year. Considering in the same manner the 56 vixens that were kept for three or more years, the per cent. that did not mate in each of the first three years were 12, 4 and 0 respectively. Clearly, failure to mate is associated with immaturity, and is only a minor problem among the adults.

Although yearlings that failed to mate improved in this respect in subsequent years, there is evidence that they were not as productive in their later years as the other vixens. Of eighteen yearlings that failed to mate, but that were kept over until their next year, 11 per cent. failed to mate as 2-year-olds. This record is not as good as that of all second-year animals, 6 per cent. of which failed to mate. In addition, the group that failed to mate during the yearling year showed up poorly in its second year with respect to non-pregnancies; 31 per cent. of this group failed to become pregnant after mating in the second season, compared with 13 per cent., the average of all second-year vixens. The number that destroyed their litters the second year was about average. The mean number of pups whelped per female during the second year was 1.6, compared with 2.2, the average for all second-year vixens. In view of this poor record in the second year, it appears that the productivity of the herd can be increased by eliminating those yearlings that fail to mate.

Discussion

Upon reviewing the various aspects of reproduction covered in this report, one is impressed by the number of instances where the reproductive process differed between yearlings and adults. Differences were found in the time of estrus, duration of copulation, maximum vulvar size and failure to mate. For each of these there was some evidence that full maturity had not been reached even in the second breeding season. The mean size of the litters whelped by yearlings was as great as that of vixens in their second and third seasons, but there was possibly an increase in litter size in the fourth year. The mean number of pups per mated female, however, did not increase with age, and old vixens were as likely to destroy their litters as were yearlings.

In some of the phases of reproduction considered in this report it has been possible to show that an individual fox tends to conform to the same pattern in several seasons. It was shown, for example, that there was a tendency for a vixen to mate at the same time in the breeding season each year. Other aspects of reproduction in which these foxes demonstrated a significant degree of individual expression were duration of copulation (definitely for males, possibly for females), vulvar size during proestrus, tendency to destroy litters, size of litters whelped by adults and possibly failure to mate and to whelp.

It is clear that the average performance of the herd could be altered, with respect to any of the characters mentioned in the preceding paragraph, by eliminating the appropriate individuals. Whether this selection would result in any permanent change in the genetic composition of the herd is unknown.

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SUMMARY

(1) The reproductive behavior of the herd of silver foxes at the U. S. Fur Animal Experiment Station, Saratoga Springs, N. Y., was studied from 1930 through 1944. Some of the observations extended over the entire period, while others were collected only in recent years.

(2) The mean mating date of the adult vixens was February 12th with a standard deviation of 9.9 days; and of yearlings, February 21st with a standard deviation of 12.0 days. The yearlings mated later than the adults every year, but not always by the same length of time; the average was 8 days later.

(3) The mean mating date of the adult herd differed by more than 2 weeks in different years. No close correlation could be found between the mean mating date of the herd and temperature of the months from September to February.

(4) There was a slight tendency for early-whelped vixens to breed earlier in their first breeding season than late-whelped vixens. This tendency was less distinct in the second season and absent in the third.

(5) There was a moderate tendency for a vixen to mate at the same part of the season each year. The coefficient of correlation between the time of estrus in the yearling year and the mean mating date in subsequent years was $+.45 \pm .07$.

(6) The mean duration of 524 copulations was 26.49 $\pm$ .46 minutes. Both males and females tended to remain coupled longer as adults than as yearlings. Individual males showed a definite tendency to remain coupled the same length of time in several copulations; females showed only a slight tendency. There was no correlation between duration of copulation and litter size.

(7) There was a definite tendency for the vulva of adult vixens to swell to the same size during proestrus in successive seasons. Vulvar size was correlated with the reproductive capacity of the vixen.

(8) There was a strong tendency for certain vixens to destroy their litters year after year.

(9) Litters whelped by yearlings were as large as those whelped by the same vixens in their second and third years. There was possibly an increase in the fourth year. There was some tendency for individual adult vixens to have the same sized litter in several seasons, but the correlation between litter size in the yearling year and subsequent years was low and of questionable significance.

(10) There was no tendency for the litters sired by an individual male to be of the same size, nor did sisters tend to bear litters of the same size.

(11) Failure to mate was more frequent among yearlings than among the same vixens in their adult years.

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HUMAN HEREDITY IN RELATION TO ANIMAL GENETICS[1]

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WHILE the animal or plant geneticist can make an analysis of related species or varieties by elaborate systems of crossing, the student of human heredity has to rely upon such data as can be derived from (1) the construction and analysis of pedigrees, (2) the comparative study of twins and (3) the use of various statistical methods, many of which have recently been developed. These, however, often lead to very definite and unassailable results. The methods of inheritance of a great range of human abnormalities are now known with a precision which sometimes vies with that of animal and plant genetics. We only have to think of the extensive literature of human physiology, anatomy, medicine and the related sciences to realize that man is better known as an organism than any other animal. Even as regards heredity, there is scarcely any organ or feature of the body of whose inheritance or variations we have no knowledge. On the other hand, in the absence of crosses to determine particular points, our knowledge of the genetics of any condition often bristles with question marks, and there are always aspects on which further data are required. However, the physiology of man is much better known than that of any animal and this helps toward an understanding of the steps by which any abnormal gene produces its effects in development.

The study of human heredity has thus reached vast dimensions. This work has two main aspects, (a) medical, which includes the investigation of the inheritance of abnormalities and the part played by constitution in disease; (b) anthropological, the analysis of the normal physical, mental and constitutional differences between

[1] Substance of a lecture at the Marine Biological Laboratory, Woods Hole, July 6, 1945.

races and within populations. Contrary to opinions still widely expressed, there is no doubt that racial characters show Mendelian segregation just like other (*unimutational*) characters, but the analysis is generally more difficult because racial characters are usually determined by polymeric factors. Writers who claim a lack of segregation in racial crosses, based on statistical anthropometric studies, have been misled through failure to include the extreme segregates in their statistics.

These two aspects of human genetics—the medical and the anthropological—might be considered to be entirely independent, but this is by no means the case. Notwithstanding their past development as separate disciplines, they dovetail in many respects. For instance, certain diseases of the blood and of the nervous system and an allergic condition such as favism are found mainly or exclusively in certain races. Cooley's anemia, also known as thalassemia, is found almost entirely in members of the Mediterranean race—Greeks, Cypriotes, Syrians, Egyptians, Italians, Sicilians. The condition is partly dominant in inheritance, homozygotes having the disease more severely than heterozygotes, and there is some evidence that the gene is in the same chromosome as the genes for the blood groups. Sicklemia is mainly if not entirely confined to Negroes in America, Africa and the West Indies. Marchiafava's disease (degeneration of the corpus callosum) is confined to Italian males. It usually appears in middle age and is frequently associated with alcoholism. Wernicke's syndrome, in which other parts of the brain are affected, appears to be the corresponding disease in peoples of Northern Europe. Favism, an allergic disease produced by eating the broad bean (*Vicia Faba*), seems to be confined to Sicilians and Southern Italians. They can contract it either in Europe or in America.

That various races and populations differ in the gene frequencies of the blood groups is well known. Various other genes, some of which are sublethal, such as haemo-

philia, appear to differ in their mutation rates in different parts of the world. It may be worth while pointing out that in so far as mutations are the material out of which new species and subspecies are constituted, changes in mutation rates must be fundamentally concerned in the origin of species. If no new mutation ever began to appear in any line of descent we should have no new species except such as might arise through the reassortment of genes already present. The origin of new characters in phylogeny implies the appearance of new mutations which had not previously occurred.

The fact that human racial characters are represented for the most part by multiple genic differences and not by single factor mutations implies a long process of evolutionary development of racial types. This view is now abundantly supported by human paleontology. When Darwin wrote the "Descent of Man," human paleontology did not exist, and he was obliged to draw his conclusions entirely from comparison of the living races with anthropoids and their relatives. He concluded that the main races of man represented subspecies of *Homo sapiens*. Since then the varied evidence from many fossil skeletons in different parts of the Old World makes it possible to fill in much of the picture of human evolution, although many points remain obscure. Not only many species but several human genera are now known to have lived since the Lower Pleistocene. However we may view their relationships and the lines of evolutionary descent, the evidence seems conclusive that much local evolution has taken place. Early populations were small and the barriers to migration by primitive man were many and formidable. In such circumstances the evolution of the Hottentot-Bushman type took place in relative isolation in South Africa, as recent discoveries in the fossil record show. Similarly the Australoid type, ending with the modern Australian aborigines, evolved from the Pithecanthropus level over a long period. Australoids have occupied at different times the whole region

from Southern India, Java and New Guinea to Australia. Remnants of this type still survive in all these regions.

By ordinary morphological standards Sinanthropus is specifically distinct from Pithecanthropus, and by the same token a consistent nomenclature for the whole group of fossil and living Hominidae necessitates recognition of the primary modern races of man as species. This subject has been discussed at greater length elsewhere (Gates, 1944). As Mayr (1942) points out, the line of distinction between species and geographic subspecies is often very indefinite. The main reason for treating the primary human races as species rather than subspecies is on the grounds of consistency and convenience, as well as the paleontological evidence for their independent origin from different ancestral stocks in widely separated areas.

This is, however, a digression from my main theme, which is a comparison of the general principles of human genetics with those of animal genetics. From this comparison a striking difference immediately emerges. A particular mutation occurring in animals or plants can generally be relied upon always to show the same type of hereditary behavior, except in case of translocation. A dominant mutation will always behave as a dominant, no matter how often it recurs.[2] This is by no means the case with human mutations. In mankind the same mutation may be consistently dominant in one family, recessive in a second and sex-linked in a third, showing the same well-known rules of inheritance through many generations in each pedigree. This is not a case of one form of inheritance being gradually transformed into another, but a gene which produces the same abnormality or essentially the same symptoms will show these different forms of hereditary behavior in different family pedigrees.

[2] Exceptions to this condition may prove to be more numerous in wild animals than has hitherto been supposed.

In animals, something of a similar kind can be seen in a few instances, but little attention has been given to them. In rabbits, Castle (1926) contrasted the dominant English spotting with the more extensive but recessive Dutch spotting and showed that their genes were not allelic but closely linked. Keeler (1931) found in mice that the genes for dominant and recessive spotting are in separate chromosomes. In horses, Kremola (1933) determined that the genes for dominant and recessive (piebald) spotting produce patterns which are complementary, the white being mainly dorsal in the former and ventral in the latter. Both dominant and recessive white spotting occur also in dogs. In Karakul sheep black wool is completely dominant to white, whereas in European breeds black is recessive.

Such pairs of dominant and recessive genes are few and mainly confined at present to spotting in domestic animals. It has been suggested that some of the peculiarities of human genetics result from man's "domestication," just as the domestication of the wild turkey has involved various changes in its instincts and habits through selection. Of about 45 genes now known to be in the X-chromosome, 13 are also known to be inherited autosomally as dominant and recessive characters. This triad of methods of behavior of particular mutations obviously requires some special explanation. Is the same gene involved in all three types of inheritance or what are the relations between the three types? Among the human abnormalities showing this triad of hereditary behavior may be mentioned megalocornea, nystagmus, retinitis pigmentosa, cataract, peroneal atrophy, albinism and psoriasis. Other conditions, such as haemophilia, which are generally sex-linked, may also rarely be an autosomal dominant or recessive. On the other hand, a white forelock, which is generally dominant, is sex-limited (like baldness) in one family; and Morquio's disease in children, which is generally recessive, is sex-linked in one pedigree and may be dominant in certain families. Dia-

betes mellitus is dominant in some families and recessive in others. From a practical point of view, since many human genes are inherited in two or three different ways, when an abnormality is being studied in a new family it often becomes necessary to investigate its inheritance *in that family*. Even blue eyes, which are universally recessive in Caucasian populations, may be dominant in Singhalese, according to recent evidence (Gates, 1938).

It is now well recognized that ordinary sex-linked genes are carried in the X-chromosome. Morgan (1910) found that the white-eyed mutation in a male of Drosophila was inherited in the manner that we now call sex-linked. Wilson (1911) showed that this inheritance followed the path of the X-chromosome from generation to generation. He also pointed out that the inheritance of color blindness in man can be explained in the same way. This discovery was one of the most significant in the history of genetics. The question arises, have these sex-linked genes in man arisen as mutations in the X-chromosome or have some of them been translocated from an autosome? Before considering this question we need to have some more information about the many cases in which the same abnormality is dominant in one pedigree and recessive in another.

Polydactyly is generally a strict dominant with never a skip, but in certain families there is some lack of penetrance. This may be due to the presence of an inhibitor in certain cases, but more likely results from a weakness of expression in the gene. In some Negro families polydactyly is either recessive or a dominant of low penetrance. Not only in polydactyly but in various other inherited conditions we thus have a sliding scale of values from complete dominance to complete recessiveness, the degree of dominance varying within relatively narrow limits in each line of descent. Such differences between pedigrees can be accounted for in part by differences in strength of the gene, but in some cases no doubt inhibitors. or an effect of the genome as a whole are involved. Cer-

tain environmental factors may also sometimes have a controlling influence. This sliding scale condition of genes, from dominant to recessive, is one of the situations which emerges from extensive study of the pedigrees of various human abnormalities. Such a condition as (recessive) leukemia is believed to show only about one per cent. of penetrance or expression in homozygotes.

There is another effect of lack of penetrance which has considerable significance. In a strictly dominant pedigree with no case of skipping a generation, the total sibships generally give a close approach to a ratio of one affected to one normal. Lack of penetrance widens this ratio, which may thus approach 1 : 15. Such ratios have sometimes been interpreted as indicating the necessity of two factors for a particular character when in reality we are dealing merely with a single factor of low penetrance.

While the line between dominant and recessive genes may thus become blurred, so that a pedigree of a dominant abnormality having low penetrance will be difficult to distinguish from a recessive, especially if the gene is not very rare, yet other characteristic differences between the dominant and recessive forms of a disease generally emerge. In such conditions as peroneal atrophy or retinitis pigmentosa the dominant form is relatively mild, the recessive form most severe and the sex-linked form intermediate in symptoms. These differences may be expressed not only in severity of symptoms but also in age of onset. Peroneal atrophy has been known since 1856 and is characterized particularly by wasting of the calf muscles. Many extensive pedigrees of the disease have now been recorded, some such families having over twenty cases, extending through four or five generations. Allan (1939) in particular has compared the three differently inherited forms in North Carolina. In a family with dominant inheritance the symptoms begin at about 30, but the individual is not incapacitated and can continue to work even at 70. The recessive sex-linked form

is more severe, the affected males being generally crippled. Muscular atrophy may begin at 15 and by 25 the victim is likely to be on crutches or even confined to bed. Most severe is the recessive form. Affected children of normal parents may begin to have difficulty in walking at 3–5 years of age and are likely to be severely crippled by the time they are 20.

Various explanations of these relationships have been suggested. The most reasonable is based on a conception of balance between the gene and its normal allele. In a dominant pedigree the affected individuals are all heterozygous, so that the gene producing the defect is balanced against its normal allele, thus making the condition milder. When the gene is recessive it is weaker than its normal allele and is therefore not expressed. One dose of the recessive gene produces little or no phenotypic effect, but two doses reinforce each other, producing a greater effect than when one dominant gene is pitted against its normal allele. Similarly, in a sex-linked recessive the affected male has a defective gene with no normal mate to balance it. The condition is therefore more severe than in a heterozygous dominant. On this reasoning, the homozygote with two doses of the dominant gene should have the condition most severely of all. In certain cases, particularly telangiectasis (Snyder and Doan, 1944) and brachyphalangy, such homozygous dominants are known to be lethal, and there is reason to conclude that this is probably true of many abnormalities, such as sebaceous cysts. But since heterozygous dominants generally marry normals there is very little direct evidence on this point.

If this view of dominant and recessive genes for the same defect is soundly based, then there must be a weaker (recessive) and a stronger (dominant) gene for many abnormalities, just as we have dominant and recessive genes for piebald spotting in various domestic animals. It would be natural to assume that the dominant and recessive genes for the same defect would be alleles, but

there is reason to believe that, in certain instances at least, this is not the case. The matter might be settled by bringing together the dominant and recessive forms of the same abnormality in one family, but at present such unions seems to be non-existent.

There is at present no explanation for the paired condition of dominant and recessive genes, but a possible explanation suggests itself. The dominant bar eye mutation in Drosophila is known to be a duplication in a segment of one chromosome. Is it possible that in each of these pairs the dominant "gene" represents a duplication of the corresponding recessive? In Romney sheep in New Zealand there is a condition of the coat known as the N-type. Dry (1944) shows that N is dominant in certain stocks, recessive in another and multifactorial in a third type derived by selection. He suggests that in the dominant form a gene duplication has taken place, the recessive being derived from it by loss of the duplication.

In Drosophila, conditions exist in certain fourth chromosome mutations which are very similar to those just outlined in man. Bent wing and cubitus interruptus occur in both dominant and recessive forms, and the dominants are lethal in the homozygous condition. Moreover, Dubinin and Sidorov (1934) found that cubitus interruptus changes from recessive to dominant in the presence of any translocation between chromosome IV and the Y-chromosome. This change in dominance is interpreted as a position effect. We thus have some reason for basing a change in dominance on translocations within the nucleus, a process which could occur in man as well as in other organisms. There is in fact some direct cytological evidence of such translocation in a descendant of a cross between French and Scottish grandparents. Two dominant and six recessive alleles are found at the eyeless locus, also in the fourth chromosome of Drosophila. Hinton (1942) has studied the interaction of these alleles. One of the dominants at least is accompanied by a duplication.

Having considered possible explanations of the existence of dominant and recessive forms of the same abnormality, how shall be account for the sex-linked form? If this has arisen through translocation from an autosome to the X, then we should find two forms of the sex-linked gene, a weaker and a stronger; for presumably the dominant and recessive forms would be equally subject to translocation. Particularly this would follow if the dominant is derived from the recessive by duplication or *vice versa*. The evidence regarding sex-linked characters which are also dominant or recessive has not been examined from this point of view, but at any rate in the case of haemophilia there is clear evidence that a milder and a more severe form (both sex-linked) exist. In some families the condition is so severe that affected sons generally bleed to death in childhood from some trifling injury, rarely surviving to reproductive age. This condition is essentially lethal. In the milder form, survival to maturity not infrequently occurs and affected males may marry and transmit the condition to half their grandsons. Hay's pedigree of a New England family, published in this country in 1813 and afterwards extended from six generations to seven by Osler, shows that four males and eleven females transmitted the condition. This distinction between a milder and a more severe form of bleeding is the real basis of the so-called laws of Nasse and of Lossen. The latter studied the Mampel family of severe bleeders and concluded that affected males could not transmit the disease. The former must have derived his evidence from the Hay pedigree in which such transmission occurs.

Medical writers have been so obsessed with the idea that haemophilia must be sex-linked that the question whether dominant or recessive haemophilia occurs is somewhat obscure. Such families will no doubt be rare, but it will not be surprising if they are found to occur. If the gene which produces haemophilia by mutation (and several such cases are on record) was originally in an

autosome, it must have been translocated to the X-chromosome long ago, at least in Europeans, because practically all such pedigrees are now sex-linked. A definitive answer to the question, "In how far are sex-linked genes in man translocated from autosomes?" will have to await a wider comparative analysis of human genetics. In the meantime it is worth pointing out that recessive sex-linked abnormalities in man are much more frequent than dominant sex-linked. In both cases there is much variability in the expression of the condition in heterozygous females, whether it be color blindness, haemophilia or thrombopathy.

Any gene such as that for haemophilia, which is sublethal or lethal in effect, must be replenished by fresh mutations or it would disappear from the population. Glioma retinae or retinoblastoma is another condition which was formerly fatal in practically every case. A tumor arises in the retina, usually in infancy, fills the eyeball and enters the brain with fatal results. The first successful operation in removal of the eye at an early stage of the glioma was performed about 1904. Either parent can transmit the condition, which appears to be a dominant, generally of low penetrance. Many single cases occur, some of which are probably fresh mutations. Various rare children's diseases, such as gargoylism and von Gierke's glycogen disease, are essentially recessive lethal genes. The parents are normal but generally related. Morquio's osseous dystrophy, first discovered in 1929, is a children's disease related to gargoylism, but differing in having more extensive skeletal changes and no mental defect. Dwarfism, hunch-back and eyes widely separated are characteristic features. The condition is generally recessive, but is strictly sex-linked in one pedigree in which 20 boys and no girls were affected in five generations. Any inherited condition with characteristic symptoms which is fatal in infancy, childhood or before the reproductive age is reached should be included among the lethal genes.

Sex-limited inheritance, which is well known in the genetics of horns in various breeds of sheep, is fairly well established as the method of inheritance of baldness in man. The gene for baldness is in an autosome, but is dominant in men and recessive in women, so that heterozygous men but only homozygous women will show the condition, although heretozygous women will transmit it to half their sons. This is a case in which sex causes a reversal of dominance. Since the gene is autosomal a father can transmit directly to his son, whereas in a sex-linked pedigree this is not possible. Certain other conditons in man are probably inherited in the same way.

In a well-known pedigree published by Cunier in 1839, color blindness is inherited only from mother to daughter through five generations. This type of hologynic inheritance probably also occurs in certain pedigrees of sex-linked ichthyosis and senile cataract, and may apply to as many as seven different conditions in particular pedigrees. The explanation was furnished by the work of Mrs. Morgan (1938), who showed that in certain strains of Drosophila the two X-chromosomes of the female are united end-to-end and pass together into the same gamete. In such human families certain females would be $\widehat{XX}Y$ while other individuals might be $\widehat{XX}X$ or YY. Since man has 48 chromosomes, these aberrations of the X and Y need not be lethal in effect; they might not even produce sterility. It is to be hoped that the new methods of counting chromosomes in the living subject will make possible the discovery and analysis of such cases. It is not impossible that triploid or even tetraploid individuals may be found in mankind, as they have been in salamanders. Chromosome numbers are known to vary widely in certain species of rats and probably vary also in dogs.

Holandric inheritance, confined to the male line from father to son, is determined by genes in the non-homologous part of the Y-chromosome. Some fourteen different conditions, ranging from the famous eighteenth century

case of ichthyosis hystrix to black hairs in the ears and hyperextensibility of the thumbs, are found with more or less certainty to be inherited in this way, at least in certain families. In a pedigree published by Davenport (1924), bilateral radio-ulnar synostosis is inherited as a holandric gene from father to son, every male (ten in 3 generations) being affected. One of the affected sons, however, had two sons affected, one son normal and one daughter affected. The normal son and affected daughter can best be explained by crossing-over of the gene from the Y to the X in two of the father's germ cells. The evidence indicates that in certain pedigrees of Leber's optic atrophy and even in one of haemophilia there can also be crossing-over of the gene between the X and the Y.

Genes in the homologous part of the X or Y can cross-over between these chromosomes and thus show partial sex-linkage, as Haldane (1936) first pointed out. Only about six genes have yet been assigned to this part of the sex chromosomes. These include total color blindness, xeroderma pigmentosum and certain forms of epidermolysis bullosa and of retinitis pigmentosa. In one pedigree of epidermolysis the gene is in the non-homologous part of the Y and therefore holandric; but there was one cross-over to the X, since a female in the family transmitted it to a son.

There is another aspect of general genetics to which human heredity has already made important contributions, namely, in the study of unitary metabolic differences. These inborn errors of metabolism now make a long list, each of which appears to be due to the absence of a particular enzyme. This absence breaks the chain of metabolic changes at a particular point, so that some substance is excreted which would normally be broken down into something else. These conditions in man include cystinuria, alkaptonuria, xyloketosuria, tyrosinosis, steatorrhoea and porphyria. Each is generally inherited as a simple recessive, but some may also be dominant or show other complications. Porphyria, which

causes great sensitiveness of the skin to light, is known not only in man but occurs as a mutation in cattle in South Africa, where it is also recessive in inheritance. In some of these derangements the symptoms are slight or nonexistent, whereas in phenylpyruvic amentia the results are very serious. One per cent. or less of mental defectives excrete phenylpyruvic acid, owing again to the absence of an enzyme. The condition is inherited as a simple recessive and the resulting mental status ranges from idiocy to mild deficiency. These persons also usually have fair hair, even in Sicilians.

Some other inherited metabolic derangements produce far-reaching effects but may have a basis almost equally simple. Gout is possible in man because of the absence of the enzyme uricase which converts uric acid into harmless allantoin. In von Gierke's glycogen disease of children there is an accumulation of glycogen in the liver and other organs. Similarly in Gaucher's disease kerasin accumulates, in xanthomatosis and Schüller-Christian disease cholesterin, in Niemann-Pick's disease lipoids.

The inheritance of this large group of metabolic derangements helps to link biochemistry with genetics. In many of these cases at least the simple absence of a particular enzyme is involved. Here is a definite relation between a gene or mutation and a particular enzyme. Extensive breeding experiments and x-ray mutations with the ascomycetous fungus, Neurospora, have shown that a whole series of enzymes controlling different chemical steps in the cell can be dropped out, each by a different mutation. It is clear that the relation between each of these enzymes and its corresponding gene must be very close, possibly even one of identity in certain cases. By such methods it has been shown that the synthesis of arginine from ornithine takes place in the same three stages in the Neurospora cell as in the liver of higher animals. Recent further study with yeasts (Spiegelman, 1945) suggests that the gene initiates the synthesis of a few molecules of such enzymes as galactozymase and

melibiozymase (which ferment galactose and melibiose respectively), their subsequent multiplication being by self-duplication of these enzyme molecules, presumably in the cytoplasm, under the influence of the substratum. In different strains of Neurospora such substances as pyridoxine, thiazole, *p*-amino benzoic acid and choline may be absent, and when these strains are crossed the differences segregate in simple Mendelian fashion. Thus it appears that many of the inherited metabolic changes in man have resulted from a simple biochemical change in the germplasm which is fundamentally genic in nature. It does not follow that morphological genic mutations are basically biochemical, although some of them may be. We know, on the contrary, as I formerly emphasized (Gates, 1931), that some are physical changes, *i.e.*, duplications, deletions, etc., in a chromosome.

A more medical aspect of human genetics is the occurrence of inherited syndromes involving two, three or more superficially independent germinal changes, all or only part of which may appear in any one individual. These syndromes are already numerous and are still multiplying in the medical literature. Possible explanations are a single gene with pleiotropic effects, or two or more linked genes, but neither of these hypotheses is always satisfactory.

A typical hereditary syndrome is arachnodactyly (spider fingers) and ectopia lentis (bilateral dislocation of the lens of the eye). Over 200 such cases have been recorded in many different families. This might be ascribed to two dominant linked genes, especially as each condition is also inherited independently in certain families. Affected persons are generally tall and slender with long fingers; they frequently have heart lesions, funnel chest, hunch-back and other mesodermal abnormalities. Their musculature is poorly developed and they may have a highly arched palate or prominent ears. This penumbra of variables surrounding the two main features makes it possible that a single pleiotropic and labile gene is involved.

Blue sclerotics, brittle bones and deafness (otosclerosis) is another well-known syndrome with dominant inheritance. Brittle bones alone are inherited in some families. The bones fracture very easily and mend quickly. In one pedigree the brittleness is confined to the bones of the arms. Otosclerosis is an irregular dominant by itself in some pedigrees and probably recessive in others. It is about twice as frequent in females as in males, which may be merely a difference in penetrance of the gene in the two sexes or may result from the combined action of a sex-linked and an autosomal gene. The triad, blue sclerotis, brittle bones and otosclerosis, sometimes appears as a single case from normal ancestors, suggesting a single mutation involving all three conditions. One, two or three of these conditions may appear in any member of a pedigree, one family having as many as 45 affected in five generations. It will be seen that the genetic problems connected with hereditary syndromes are far from settled and require much further investigation. They are more fully discussed in my forthcoming work on "Human Genetics."

Another condition which has been much studied is known as the Laurence-Moon-Bardet-Biedl syndrome, after those who described the first cases. The original paper of Laurence and Moon dates from 1866, and cases have since been described not only from various parts of Europe and America but also from Egypt and Japan. Later studies disclose a pentad of conditions. Retinal dystrophy (which resembles retinitis pigmentosa) and obesity are always present. In addition genital dystrophy, mental defect and polydactyly are of frequent occurrence in affected individuals. From the published pedigrees it is possible that two linked recessive genes are involved, one producing the ectodermal defects, *i.e.*, retinal dystrophy and mental deficiency, the other producing the three mesodermal abnormalities. The obesity, polydactyly and mental defect are all visible in childhood. In some of these syndromes inherited endocrine dysfunc-

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tion is probably involved as an intermediary between the gene and its full expression.

The aspects of investigation in human genetics which have been briefly touched upon will suffice to show the value of experimental genetics in supplying analogical data for the interpretation of heredity in mankind. They also show that the study of human heredity can make important contributions to general genetics in its own right.

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SHORTER ARTICLES AND DISCUSSION

LINKAGE RELATIONS OF FLEXED-TAIL IN PEROMYSCUS[1]

SOME time ago one of us reported, in a joint paper with Elizabeth Barto, a brief description of a tail anomaly in *Peromyscus maniculatus* which was given the provisional name of flexed-tail (Huestis and Barto, 1936). A small tabulation of F_2 and test-cross progeny results, presented at the same time, made it probable that the tail anomaly was due to a recessive gene substitution. This had presumably taken place in a wild mouse, some carrier descendant of which was trapped at Silver Lake, Oregon. A chance mating of cage-bred relatives of the trapped mouse brought the character to notice: two flexed mice appearing in a brood of three.

Routine tests were made with other available mutant genes with no noticeable indication of linkage except in the tests with silver pelage. In this case a thousand F_2 mice were reared before a silver-flexed individual was obtained. Test crosses of F_1 females in repulsion phase made possible by the use of this silver-flexed mouse, a male, gave indication of linkage of the genes being investigated. The cross-over percentage computed from this test cross progeny was 22 ± 4.03 per cent. (Heustis and Piestrak, 1942). Since this would not account for the scarcity of F_2 double recessives unless the cross-over percentage in F_1 males was very low, the latter point was investigated and reported upon briefly elsewhere (Huestis, 1943).

This paper presents the figures now available upon reasonably comprehensive tests of the cross-over incidence between flexed-tail and silver pelage in repulsion and coupling phases in both female and male parent mice. We are also including figures computed from an F_2 and two test cross progeny groups which indicate that flexed-tail is also linked with albinism.

Various exigencies have postponed a comprehensive search for other bodily responses of mice to the gene that produces flexed tails. A number of facts have accumulated during the routine

[1] This research has been assisted by grants in aid from the American Philosophical Society and the Research Council of the Oregon State System of Higher Education.

breeding work, however, which will receive a brief discussion before comments are made on the linkage data.

The Flexed Character

Besides the difference in linkage relations (flexed-tail in the house mouse is not in linkage group 1) (Snell, 1941), the initial idea that flexed tail in Peromyscus would prove to be developmentally homologous with the similarity named character in Mus has not been substantiated. A noticeable feature of the character in the latter genus is anemia of the newborn (Hunt, Mixter and Perman, 1933). Flexed-tail Peromyscus young are not noticeably paler than normal-tailed litter mates. We have also made enough hemometer (and Talquist scale) measurements of the blood of flexed-tailed and normal Peromyscus under two weeks of age to be able to state that if there is a difference in the per cent. of hemoglobin it could only be demonstrated by a comparison of very large series.

The gene for flexed-tail in Peromyscus produces disalignment of tail vertebrae, especially those of the distal half of the tail. It shortens the tail. It may produce a clockwise or anti-clockwise rotation. In extreme cases this may take place through 180 degrees so the ventral side of the tail is uppermost. When this rotation is combined with a flex near the base and the mouse picks up some mites these may be found in their usual position at the base of the ventral side of the tail although it is now dorsally oriented. The gene for flexed-tail does not, however, reduce the body size of the adult mice in which it shortens the tail. This has been shown by a comparison of the measurements of 150 recently killed specimens, fifty of which were flexed. Measurements of other dimensions on the same mice show that flexed-tailed mice do not differ from non-flexed in the length of the usually measured appendages; the foot and the ear.

Measurements show, in addition to the facts outlined above, that the gene for flexed-tail is an almost complete recessive as far as any shortening of the tail is concerned. F_1 hybrid mice have a relative tail length which is 2 per cent. less than non-flexed mice, but this difference, based upon 100 individuals, is not quite two times its standard error. A statistical presentation of these data will be published elsewhere.

Linkage

There are a number of factors which make it difficult to get quit comparable figures for linkage in the two sexes. Both characters overlap the normal type; silver a little and flexed somewhat more. Most of the "dark" silvers can probably be identified by microscopic examination of the hairs. We make this observation as a routine in doubtful cases. Tests of a somewhat inbred group of albino flexed mice showed that 10 per cent. of the young of flexed parents can not be identified as flexed by examination of the skin and pelage covered tail. While this gives us a figure to keep in mind it can not be used as a constant correction factor, for other groups have been shown to differ in the percentage of overlaps. Parents with slight flexes tend to throw more overlaps than those with very flexed tails, but there are individual differences among parents, with a similar phenotype, in the percentage of overlaps thrown.

Tails that are slightly flexed are commonest; the number of individual mice found being in inverse order to the expression of the character as judged by counts of flexes that can be seen and felt through the skin of the tail. For example, in 50 flexed mice taken at random the number of mice with one, two, three, four or five flexes was 19, 15, 12, 3 and 1, respectively.

In mating routine we use a few males and relatively many females. Since only a few flexed male parents need be selected for linkage tests they will, on an average, be better flexed than flexed female parents. They will then presumably throw fewer overlaps than flexed female parents, in which group we are obliged by consideration of time to use all available individuals. Thus female cross-over percentages should be based on fewer overlaps and be more accurate than male cross-over percentages. Beides this sex difference based on parent selection, there is the fact that overlaps increase the calculated crossing-over percentage. This is because some overlapping phenotypes which should be in a cross-over class will be identified in a non-cross-over class and some overlapping phenotypes which should be recorded as non-cross-overs will appear in a cross-over class. But cross-over types being numerically the smaller groups will, on a percentage basis, always gain more than they give. The smaller the percentage of crossing-over the larger the gain will be. Thus male cross-over percentages, if smaller, will gain more than female

cross-over percentages. With 20 per cent. crossing-over and 10 per cent. of flexed overlaps, four flexed silver overlaps in 100 young would be recorded as silver and one flexed mouse recorded as wild type. This difference will occasion a gain of three mice to a cross-over class. In this case 20 per cent. crossing over would be calculated at 23 per cent. Similarly 10 per cent. crossing over would be recorded as 14 per cent., a larger error and relatively a much larger gain for a cross-over class.

Flexed overlaps recorded as non-flexed may sometimes be used as parents being mistaken for double heterozygous genotypes. They will throw many (although not all) flexed young. In the case of Peromyscus females, which do not produce many young, the mother's genotype can not be statistically determined from this fact with any great degree of probability. Examination of the records of male parents with twenty or more offspring showed one repulsion phase individual whose record could with considerable probability be interpreted as being due to this error in parent selection. Sixty-five per cent. of his 40 test cross young were flexed. These have not been included in the tables we are presenting. There was another possible case. We have no critical data available with which to estimate the effect of chance choosing of overlap phenotypes for parents.

Between the date of birth and the date of phenotype identification, which must be at least three weeks later when silver progeny are to be examined, some loss of young occurs. This loss, in our experience, is more liable to occur in the mutant phenotypes and this will alter the balance of test cross classes. The percentage of silver phenotypes was apparently not greatly altered in these test crosses by differential viability or overlapping, since 49 per cent. of 866 mice was silver when 50 per cent. was expected. Forty-four per cent. of the 866 was flexed when 50 per cent. was expected. This represents a 12 per cent. loss presumably due to identification of overlapping phenotypes as non-flexed young, and differential viability.

The data are assembled in Table 1. Crossing-over percentages in the production of female gametes are reasonably similar in the two linkage phases, the difference between them being 2.2 ± 3.64 per cent.

The percentages of crossing over in the males are smaller than in the females, as is expected in the heterogametic sex. Male coupling and repulsion phases exhibit a cross-over difference of

8.0 ± 4.69 per cent. This difference is almost twice its standard error and has not been accounted for. There are also some other progeny class irregularities which, while they do not greatly lessen the probability of linkage, represent somewhat disconcerting effects of factors which we would like to eliminate from our breeding tests.

TABLE 1

TEST CROSSES OF F_1 MICE MATED WITH DOUBLE RECESSIVES

Linkage phase	Sex	Offspring					
		Not flex or silver	Flexed	Silver	Flexed silver	N.	Per cent. cross-over
Coupling F_1	♀	138	41	60	126	365	27.7 ± 2.34
Repulsion F_1	♀	34	63	98	21	216	25.5 ± 2.96
Coupling F_1	♂	84	17	7	57	165	14.5 ± 2.73
Repulsion F_1	♂	18	46	47	9	120	22.5 ± 3.81

LINKAGE OF FLEXED TAIL AND ALBINISM

We have not as many tests or as many individuals with which to demonstrate this linkage as was the case with flexed-tail and silver pelage but each of three different experiments exhibit the probability of linkage. These experiments include an F_2 progeny from parents in repulsion phase and two test-cross progenies; one from each sex in repulsion phrase. The data are tabulated with chi square calculations in Table 2. Crossover percentages are 40.7 ± 4.4 in females and 30.0 ± 5.5 in males.

TABLE 2

CHI SQUARE DETERMINATIONS OF AN F_2 AND TWO OTHER REPULSION PHASE TESTS WHICH SHOW LINKAGE OF FLEXED-TAIL AND ALBINISM

Class	Obs. freq. (x)	Exp. freq. (m)	Deviation	$\frac{(x-m)^2}{m}$	Linkage phase
Not flex or alb.	47	55.1	− 8.1	1.197	
Flexed	23	18.4	+ 4.6	1.164	F_2 Repulsion
Albino	26	18.4	+ 7.6	3.164	
Alb. flex.	2	6.1	− 4.1	2.778	
	98	98.0	0.0	8.303	P > .05
Not flex or alb.	34	30.75	+ 3.25	.343	
Flexed	43	30.75	+12.25	4.880	Female
Albino	30	30.75	− .75	.018	Repulsion
Alb. flex.	16	30.75	−14.75	7.075	
	123	123.00	0.0	12.316	P > .01
Not flex or alb.	14	17.5	− 3.5	.700	
Flexed	26	17.5	+ 8.5	4.128	Male
Albino	23	17.5	+ 5.5	1.728	Repulsion
Alb. flex.	7	17.5	−10.5	6.300	
	70	70.0	0.0	12.856	P > .01

The probability that flexed-tail is linked with albinism gives us four known gene loci on chromosome 1 of *Peromyscus maniculatus:* albinism, pink eye dilution, silver pelage and flexed-tail (Barto, 1941). Albinism is epistatic to pink eye dilution and both albinism and pink eye dilution are epistatic to silver pelage and all other known pigment characters. Since flxed-tail is neither epistatic nor hypostatic to any of the others it will be the easiest locus to use in testing for the chromosomal position of other gene loci.

Summary

1. Flexed-tail in *Peromyscus maniculatus* is linked with silver pelage.

2. The cross-over percentages calculated from female repulsion and coupling phases were 25.5 and 27.7 per cent., respectively.

3. The cross-over percentages calculated from male repulsion and coupling phases were 22.5 and 14.5 per cent., respectively.

4. It was shown how these calculated percentages have probably been increased by the presence of normal overlaps among the flexed genotypes.

5. Flexed tail in *Peromyscus maniculatus* is linked with albinism.

6. The calculated percentages of crossing-over were 40.7 per cent. in females and 30.0 per cent. in males, both sexes having the linked genes in repulsion phase.

7. Chromosome 1. of *Peromyscus maniculatus* now has four known loci: albinism, pink eye dilution, silver pelage and flexed-tail.

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DOMINANCE AND HETEROSIS

When Mendel introduced the terms "dominant" and "recessive" he was concerned with single gene differences where the heterozygote so closely resembled one of the true breeding parental forms in character that the other "either escapes observation completely or can not be detected with certainty" (translation in Bateson, 1909). Thus dominance, as Mendel observed and described it, was complete or virtually so. This acceptance of complete dominance as the standard for judging the relation of heterozygous and homozygous phenotypes is emphasized by Bateson's (1909) use of the term "imperfect dominance" to describe the situation where the heterozygote is manifestly intermediate in character. As used in this way the concept is precise and departures from dominance are readily recognizable.

Even, however, leaving aside the special use made of the term dominant by the Drosophilists in describing mutant genes, dominance has now come to be employed in a wider connotation. This is especially true in the study of quantitative inheritance where the genetic differences must be attributed to the simultaneous action of a number of genes none of which is consistently distinguishable from its fellows in experiment. Moreover, the standard of reference has been moved from that of complete dominance to the level where dominance is absent. These changes though gradual have not been made without confusion. It is therefore profitable to examine in a brief way some of their consequences and implications.

In the first place where several genes are either known or may be supposed to be acting, as is the case with quantitative characters, the "dominance" of the set of genes derived from one parent over those from the other must be a compound phenomenon. As Powers (1944) points out, "both intra-allelic and inter-allelic interactions may be involved": the observed "dominance" is not a property of any one gene, but of all the genes acting as a set. In this usage the term loses much of its precision, for clearly

even where inter-genic interaction is absent, the observable "dominance" is the average of the dominances of the individual genes. There is no reason to suppose that these genes will all show dominance either to the same degree or in the same direction. Indeed, both theoretical considerations and such observations as have been made suggest that the dominance relations of the individual genes affecting one character may be varied (see Mather, 1943). Where inter-genic interaction also comes into play the situation must be even more complex.

There is, therefore, much to be said for restricting the term dominance to the description of single gene relations, and using a separate term to describe cases where several genes are involved. Wigan (1944) has proposed the term "potence" for the integrated dominance effect of all the genes in such cases.

This distinction is all the more important as it is possible to investigate, at least within certain limits, the dominance relations of individual genes producing similar and analytically indistinguishable effects, as well as to observe their joint potence (Fisher, Immer and Tedin, 1932). For our present purpose, however, it is sufficient to note that although dominance of individual genes need not result in potence of a set of them (since dominance in the two directions may be balanced), the occurrence of potence in any direction must imply a preponderance of dominance in that direction, except perhaps in cases with special types of inter-genic interaction. Thus the observation of potence must be held generally to include the observation of corresponding dominance, though the converse is not true.

Secondly, we must consider the transference of the standard of reference from complete dominance to that of no dominance. The former is not difficult to define sufficiently well for ordinary use, as Mendel showed. It demands no scale of measurement because only two classes of gene are recognized, those which are dominant and those which are, in Bateson's terminology, imperfectly so. We can, of course, attempt to discriminate degrees of imperfect dominance, whereupon a scale becomes necessary, but this is not essential to the definition of the starting point.

The absence of dominance, however, presents more difficulties of definition. It is easy to see that the F_1 should be midway between the two parents, but this is not of itself a sufficient definition. When the heterozygote is intermediate between the two homozygotes, either allelomorph may easily be judged to be

partially dominant or dominance may be judged to be absent, according to the scale we choose for measuring the expression of the character by means of which the genic difference is detected. To take an example, suppose that the character shows expressions of 1 and 9 in the two homozygotes, and 4 in the heterozygote, on the scale we happen to find convenient for measurement. Then on this scale the allelomorph making for lower values must be judged partially dominant, as 5 is the point midway between 1 and 9. If, however, we suspect that gene action is geometrically rather than arithmetically cumulative, and consequently take logarithms of our observations (this being a common practice in the study of quantitative inheritance) values of 0, 0.60 and 0.95 are obtained, whereupon it seems clear that the allelomorph making for higher values is partially dominant. Finally, if neither of these scales seems appropriate and we transform the metric by taking square roots, the series becomes 1, 2 and 3. Dominance now appears to be absent. Similar examples can be developed at will.

Thus before we can define the midway point, at which dominance is absent, it is necessary to decide on the scale or metric which is to be used in representing the degrees of expression of the character in question. It is clearly unsafe, in the absence of other justification, to regard any scale, which we may find convenient for use with the measuring instruments at our disposal, as appropriate to the representation of the action of the genes involved: the organisms need not develop in accordance with any systems we choose to consider. Indeed we have no reason to suppose that all the genes affecting a character operate on the same scale. There is therefore no *a priori* method of scaling. It is necessary to use the scale which seems analytically most appropriate to the character under investigation, remembering always that at best this scale, when found, may only represent some combination or integral of the various scales on which different genes are in fact operating. This principle is tacitly recognized in the discussions in various investigations of quantitative inheritance as to whether the arithmetic scale used in taking the measurements or its logarithmic transformation is the more appropriate for measuring character expression. But it is necessary to go beyond the point reached by these discussions and to realize, as Powers (1941) has done in his tomato investigations, that neither the arithmetic nor logarithmic scales may be appropriate, with the

corollary that some other transformation of the data would be more valuable.

This, of course, raises the question of how a scale appropriate to the detection and measurement of dominance can be discovered. Quite obviously dominance relations themselves can not be made the basis of the method of scaling. Where, however, several genes are acting, as in quantitative inheritance, scaling can be undertaken on the basis of inter-genic relations. The most reasonable scale to adopt would appear to be that on which the average effect of any gene substitution is constant over the range of variation of character expression. This demands that the individual effects of the various genes are simply additive on the scale in question. Since non-additiveness of genic effects is a main source of what Fisher, Immer and Tedin (1932) and Mather (1943) term metrical bias, an appropriate scale could be achieved by finding a transformation which eliminated or at least minimized metrical bias.

If genic interactions of the complementary or epistatic types are regarded as being involved, it may be impossible to find a wholly satisfactory transformation; but provided we are prepared to accept an average transformation the problem would seem capable of solution. It may be remarked too, that no quantitative data from breeding experiments have yet been shown to be incapable of a transformation which would serve to eliminate metrical bias. The possibility of such genic interaction does, however, serve to emphasize that even a transformation satisfactory for the analysis in hand need not do more than lead to an approximation of the scale or scales on which the genes are working. We must therefore be cautious in drawing conclusions about the physiology of gene action and interaction from such calculations.

With all its possible imperfections such a method of scaling does provide us with an objective means of defining the situation where dominance is absent sufficiently well for practical use. It is the situation where for any gene the heterozygote is, on this scale, midway in expression between the two homozygotes, observations being made on all three genetic types over the same range of environments. The use of a scale which removes metrical bias has the further theoretical advantage, in the study of variability, that the "genotypic" variation (Fisher, 1930) is thereby reduced in relation to the basic "genetic" variation, on which the breed-

ing potentialities of the organism depend. Prediction of breeding behavior thus becomes more accurate.

In his consideration of yield in maize Powers (1941) finds that the yields themselves show no marked metrical bias, and hence rescaling is unnecessary. Data on various quantitative characters in tomato crosses, however, were found to show marked metrical bias. This was largely removed by a logarithmic transformation, though even then a small residual bias was still detectable. These logarithmically transformed figures, though not wholly free from bias, are clearly, on our criterion, to be preferred for the consideration of dominance relations.

This is also true of the tomato examples cited by Powers (1944) where he considers yield as the *product* of two quantitative sub-characters, ripe fruit number and size of fruit. One of these examples (cross 4102 × 4110) shows heterosis for fruit yield, though the sub-characters when taken separately show potence, and hence preponderant dominance, in the direction of reduced number and size of fruit on the untransformed scale. If this scale is accepted as appropriate it means, as Powers points out, that dominance in a particular direction is not a necessary condition for the occurrence of heterosis in that direction. Before, however, this conclusion is finally accepted and Jones's hypothesis, that heterosis is the outcome of dominance of the controlling genes, is modified, it is worth considering the effects of logarithmic transformation, which, as we have seen, largely removes metrical bias.

It is unfortunately impossible to make a rigorous examination on any transformed scale, as Powers presents his data in the form of means and their standard errors; but something can be learned of the effects of logarithmic transformation in modifying our judgment on the question of potence and dominance.

TABLE 1

	Ripe fruit	Size	Yield
4102	0.64	2.14	2.78
F_1	1.65	1.74	3.39
4110	2.04	1.23	3.27
Mean of parental values	1.34	1.69	

Table 1 gives the logarithms of the parental and F_1 means in the cross 4102 × 4110 from Powers's Table 1. Yield, of course, still shows heterosis; but with both sub-characters the log of the F_1 mean is higher than the mean of the transformed parental

values. Some caution is necessary in interpreting these results, because the log of the mean is not the same as the mean of the logs unless individual values are identical. Where the logarithms are normally distributed the log of the mean will be somewhat greater than the mean of the logs, the difference being a function of the variance of the distribution of the logs. It is impossible to say precisely how far this affects the conclusions to be drawn from Table 1; but it at least seems likely that in the case of fruit size there would be no potence and preponderant dominance of small size, and that with fruit number potence and dominance would remain towards greater number on this transformed scale.

Thus, so far as can be determined from the data as presented, the preponderance of dominance goes hand in hand with heterosis on the scale obtained by a transformation which greatly reduces metrical bias. A transformation which eliminated bias would appear likely to change the situation only in that it would emphasize the preponderance of dominance of greater number and perhaps even show a similar effect of greater size. It would therefore be unwise to conlude that a body of data demands a modification of Jones's hypothesis until it has been shown that the transformation necessary to remove, or at least minimize, metrical bias still leaves dominance either absent or preponderantly opposed in direction to the observable heterosis. Indeed the use of an appropriate method of scaling would appear to be a first step essential to the consideration of dominance or any other phenomenon in data on quantitative inheritance.

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